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CONTENTS:

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*THE RECENT DEVELOPMENT OF BIOLOGY.**

I.

THE task allotted to me on this occasion is a review of the development of biology during the last century. The limited time at our disposal will necessitate many omissions and will force me to confine myself to the discussion of a few of the departures in biology which have led or promise to lead to fertile discoveries.

The problem of a scientific investigator can always be reduced to two tasks; the first, to determine the independent variables of the phenomena which he has under investigation, and secondly, to find the formula which allows him to calculate the value of the function for every value of the variable. In physics and chemistry the independent variables are in many cases so evident that the investigation may begin directly with the quantitative determination of the relation between the change of the essential variable and the function. In biology, however, the variables, as a rule, can not be recognized so easily and a great part of the mental energy of the investigators must be spent in the search for these variables. To give an example, we know that in many eggs the development only begins after the entrance of a spermatozoon into the egg. The spermatozoon must produce some kind of a change in the egg, which is responsible for the development. But we do not know which variable in the egg is changed by the spermatozoon, whether the latter pro-

* Address delivered at the Congress of Arts and Science in St. Louis.

duces a chemical or an osmotic change, or whether it brings about a change of phase or some other effect. It goes without saying that a theory of sexual fertilization is impossible until the independent variable in the process of sexual fertilization is known.

But the investigations of the biologist differ from those of the chemist and physicist in that the biologist deals with the analysis of the mechanism of a special class of machines. Living organisms are chemical machines, made of essentially colloidal material which possess the peculiarity of developing, preserving and reproducing themselves automatically. The machines which have thus far been produced artificially lack the peculiarity of developing, growing, preserving and reproducing themselves, though no one can say with certainty that such machines might not one day be constructed artificially.

The specific and main work of the biologist will, therefore, be directed toward the analysis of the automatic mechanisms of development of self-preservation and reproduction.

II.

THE DYNAMICS OF THE CHEMICAL PROCESSES IN LIVING ORGANISMS.

The progress made by chemistry, especially physical chemistry, has definitely put an end to the idea that the chemistry of living matter is different from the chemistry of inanimate matter. The presence of catalyzers in all living tissues makes it intelligible that in spite of the comparatively low temperature at which life phenomena occur the reaction velocities for the essential processes in living organisms are comparatively high. It has been shown, moreover, that the action of the catalyzers found in living organisms can be imitated by certain metals or other inorganic catalyzers. We may, therefore, say that it is now proved beyond all doubt that the variables in the chemical processes in living organisms are identical with those with which the chemist has to deal in the laboratory. As a consequence of this result chemical biology has during the last years entered into the series of those sciences which are capable of predicting their results quantitatively. The application of the theory of chemical equilibrium to life phenomena has led biological chemists to look for reversible chemical processes in living organisms and the result is the discovery of the reversible enzyme actions, which we owe to A. C. Hill. I think it marks the beginning of a new epoch of the physiology of metabolism that we now know that the same enzymes not only accelerate the hydrolysis, but also in some cases, if not generally, the synthesis of the products of cleavage. It is not impossible that the results thus obtained in the field of biology will ultimately in return benefit chemistry, inasmuch as they may enable chemistry to accomplish syntheses with the help of enzymes found in living organisms which could otherwise not be so easily obtained.

A very beautiful example of the conquest of biological chemistry through chemical dynamics is offered by the work of Arrhenius and Madsen. These authors have successfully applied the laws of chemical equilibrium to toxins and anti-toxins so that it is possible to calculate the degree of saturation between toxins and antitoxins for any concentration with the same ease and certainty as for any other chemical reaction.

We know as yet but little concerning the method by which enzymes produce their accelerating effects. It seems that the facts recently gathered speak in favor of the idea of intermediary reactions. According to this idea the catalyzers participate in the reaction, but form combinations that are again rapidly decom-

posed. This makes it intelligible that at the end of the reaction the enzymes and catalyzers are generally in the same condition as at the beginning of the reaction, and that a comparatively small quantity of the catalyzer is sufficient for the transformation of large quantities of the reacting substances.

This chapter should not be concluded without mentioning the discovery of zymase by Buchner. It had long been argued that only certain of the fermentative actions of yeast depended on the presence of enzymes which could be separated from the living cells, but that the alcoholic fermentation of sugar by yeast was inseparably linked together with the life of the cell. Buchner showed that the enzyme which accelerates the alcoholic fermentation of sugar can also be separated from the living cell, with this purely technical difference only, that it requires a much higher pressure to extract zymase than any other enzymes from the yeast cell.

III.

PHYSICAL STRUCTURE OF LIVING MATTER.

We have stated that living organisms are chemical machines whose framework is formed by colloidal material consisting of proteins, fatty compounds, and carbohydrates. These colloids possess physical qualities which are believed to play a great rôle in life phenomena. Among these qualities are the slow rate of diffusion, the existence of a double layer of electricity at the surface of the dissolved or suspended colloidal particles, and the production of definite structures when they are precipitated. We may consider it as probable that the cytological and histological structures of living matter will be reduced to the physical qualities of the colloids. But, inasmuch as the physics of the colloids is still in its beginning, we must not be surprised that the biological application of its results is still in the stage of mere suggestions. The most important result which has thus far been accomplished through the application of the physics of colloids to biology is Traube's invention of the semipermeable membranes. To Traube we owe the discovery that every living cell behaves as if it were surrounded with a surface film which does not possess equal permeability for water and the substances dissolved in it. Salts which are dissolved in water, as a rule, migrate much more slowly into the living cells than water. This discovery of the semi-permeability of the surface films of living protoplasm made, it possible to recognize the variable which determines the exchange of liquids between protoplasm and the liquid medium by which it is surrounded, namely, the osmotic pressure. Inasmuch as the osmotic pressure is measurable, this field of biology has entered upon a stage where every hypothesis can be tested exactly and biology is no longer compelled to carry a ballast of shallow phrases. We are now able to analyze quantitatively such functions as lymph formation and the secretion of glands.

Recent investigations have thrown some light on the nature of the conditions which seem to determine the semi-permeability of living matter. Quincke had already mentioned that a film of oil acts like a semipermeable membrane. From certain considerations of surface tension and surface energy it follows that every particle of protoplasm which is surrounded by a watery liquid must form an extremely thin film of oil at its surface. Overton has recently shown that of all dissolved substances those which possess a high solubility in fat, *e. g.*, alcohol, ether, chloroform, diffuse most easily into living cells. Overton concludes that lipoid substances such as lecithin and cholesterin which are found in every cell determine the phenom-

enon of the semi-permeability of living matter.

IV.

DEVELOPMENT AND HEREDITY.

We now come to the discussion of those phenomena which constitute the specific difference between living machines and the machines which we have thus far been able to make artificially. Living organisms show the phenomena of development. During the last century it was ascertained that the development of an animal egg, in general, does not occur until a spermatozoon has entered it, but, as already stated, we do not know which variable in the egg is changed by the spermatozoon. An attempt has been made to fill the gap by causing unfertilized eggs to develop with the aid of physicochemical means. The decisive variable by which such an artificial parthenogenesis can be best produced is the osmotic pressure. It has been possible to cause the unfertilized eggs of echinoderms, annelids and mollusks to develop into swimming larvæ by increasing transitorily the osmotic pressure of the surrounding solution. Even in vertebrates (the frog and petromyzon), Bataillon has succeeded in calling forth the first processes of development in this way. In other forms specific chemical influences cause the development, *e. g.*, in the eggs of star-fish diluted acids and, best of all, as Delage has shown, carbon dioxide. In the eggs of *Chætopterus* potassium salts produce this result and in the case of *Amphitrite*, calcium salts.

From a sexual cell only a definite organism can arise whose properties can be predicted if we know from which organism the sexual cell originates. The foundations of the theory of heredity were laid by Gregory Mendel in his treatise on the 'Hybrids of Plants,' one of the most prominent papers ever published in biology. Mendel showed in his experiments that certain simple characteristics, as, for example, the round or angular shape of the seeds of peas or the color of their endosperm is already determined in the germ by definite determinants. He showed, moreover, that in the case of the hybridization of certain forms one half of the sexual cells of each child contains the determinants of the one parent, the other half contains the determinants of the other parent. In thus showing that the results of hybridization can be predicted numerically not only for one but for a series of generations, according to the laws of the calculus of probability, he gave not a hypothesis, but an exact theory of heredity. Mendel's experiments remained unnoticed until Hugo de Vries discovered the same facts anew, and at the same time became aware of Mendel's treatise.

The theory of heredity of Mendel and de Vries is in full harmony with the idea of evolution. The modern idea of evolution originated, as is well known, with Lamarck, and it is the great merit of Darwin to have revived this idea. It is, however, remarkable that none of the Darwinian authors seemed to consider it necessary that the transformation of species should be the object of direct observation. It is generally understood in the natural sciences either that direct observation should form the foundation of our conclusions or mathematical laws which are derived from direct observations. This rule was evidently considered superfluous by those writing on the hypothesis of evolution. Their scientific conscience was quieted by the assumption that processes like that of evolution could not be directly observed, as they occurred too slowly, and that for this reason indirect observations must suffice. I believe that this lack of direct observation explains the polemical character of this literature, for wherever we can base our conclusions upon direct observations

polemics become superfluous. It was, therefore, a decided progress when de Vries was able to show that the hereditary changes of forms, so-called 'mutations,' can be directly observed, at least in certain groups of organisms, and secondly, that these changes take place in harmony with the idea that for definite hereditary characteristics definite determinants, possibly in the form of chemical compounds, must be present in the sexual cells. It seems to me that the work of Mendel and de Vries and their successors marks the beginning of a real theory of heredity and evolution. If it is at all possible to produce new species artificially I think that the discoveries of Mendel and de Vries must be the starting point.

It is at present entirely unknown how it happens that in living organisms, as a rule, larger quantities of sexual cells begin to form at a definite period in their existence. Miescher attempted to solve this problem in his researches on the salmon. But it seems that Miescher laid too much emphasis upon a more secondary feature of this phenomenon, namely, that the sexual cells in the salmon apparently develop at the expense of the muscular substance of the animal. According to our present knowledge of the chemical dynamics of the animal body it seems rather immaterial whether the proteins and other constituents of the sexual cell come from the body of the animal or from the food taken up. The causes which determine the formation of large masses of sexual cells in an organism at a certain period of its existence are entirely unknown.

A little more progress has been made in regard to another problem which belongs to this group of phenomena, namely how it happens that in many species one individual forms sperm, the other eggs. It has been known for more than a century that it is possible to produce at desire either females exclusively or both sexes in plant lice. In bees and related forms, as a rule at least, only males originate from the unfertilized eggs; from the fertilized eggs only females. It is, moreover, known that in higher vertebrates those twins which originate from one egg have the same sex, while the sex of twins originating from different eggs may be different. All facts which are thus far known in regard to the determination of sex seem to indicate that the sex of the embryo is already determined in the unfertilized egg, or at least immediately after fertilization. I consider it possible that in regard to the determination of sex, just as in the case of artificial parthenogenesis, a general variable will be found by which we can determine whether an egg cell will assume male or female character.

V.

INSTINCT AND CONSCIOUSNESS.

The difference between our artificial machines and the living organisms appears, perhaps, most striking when we compare the many automatic devices by which the preservation of individuals and species is guaranteed. Where separate sexes exist we find automatic arrangements by which the sexual cells of the two sexes are brought together. Wherever the development of the eggs and larvæ occurs outside of the body of the mother or the nest we often find automatic mechanisms whereby the eggs are deposited in such places as contain food on which the young larva can exist and grow. We have to raise the question how far has the analysis of these automatic mechanisms been pushed. Metaphysics has supplied us with the terms 'instinct' and 'will' for these phenomena. We speak of instinct wherever an animal performs, without foresight of the ends, those acts by which the preservation of the individual or the species is secured. The term 'will' is reserved for those cases where these proc-

esses form constituents of consciousness. The words 'instinct' and 'will' do, however, not give us the variables by which we can analyze or control the mechanism of these actions. Scientific analysis has shown that the motions of animals which are directed towards a definite aim depend upon a mechanism which is essentially a function of the symmetrical structure and the symmetrical distribution of irritability. Symmetrical points of the surface of an animal, as a rule, have the same irritability, which means that, when stimulated equally, they produce the same quantity of motion. The points at the oral pole as a rule possess a qualitatively different or greater irritability than those at the aboral pole. If rays of light or current curves, or lines of diffusion or gravitation, start from one point and strike an organism, which is sensitive for the form of energy involved, on one side only, the tension of the symmetrical muscles or contractile elements does not remain the same on both sides of the body, and a tendency for rotation will result. This will continue until the symmetrical points of the animal are struck equally. As soon as this occurs there is no more reason why the animal should deviate to the right or left from the direction of its plane or axis of symmetry. These phenomena of automatic orientation of animals in a field of energy have been designated as tropisms. It has been possible to dissolve a series of mysterious instincts into cases of simple tropisms. The investigation of the various cases of tropism has shown their great variety and there can be no doubt that further researches will increase the variety of tropisms and tropism-like phenomena. I am inclined to believe that we possess in the tropisms and tropism-like mechanisms the independent variable of such functions as the instinctive selection of food and similar regulatory phenomena.

As far as the mechanism of consciousness is concerned no scientific fact has thus far been found that promises an unraveling of this mechanism in the near future. It may be said, however, that at least the nature of the biological problem here involved can be stated. From a scientific point of view we may say that what we call consciousness is the function of a definite machine which we will call the machine of associative memory. Whatever the nature of this machine in living beings may be, it has an essential feature in common with the phonograph, namely, that it is capable of reproducing impressions in the same chronological order in which they come to us. Even simultaneous impressions of a different physical character, such as, for instance, optical and acoustical, easily fuse in memory and form an inseparable complex. The mechanism upon which associative memory depends seems to be located, in higher vertebrates at least, in the cerebral hemispheres, as the experiments of Goltz have shown. The same author has shown, moreover, that one of the two hemispheres suffices for the efficiency of this mechanism and for the full action of consciousness. As far, however, as the physical or chemical character of the mechanism of memory is concerned, we possess only a few starting points. We know that the nerve cells are especially rich in fatty constituents and Overton and Hans Meyer have shown that substances which are easily soluble in fat also act as very powerful anesthetics, for instance, chloroform, ether and alcohol, and so on. It may be possible that the mechanism of associative memory depends in some way upon the constitution or action of the fatty compounds in our nerve cells. Another fact which may prove of importance is the observation made by Speck that if the partial pressure of oxygen in the air falls below one third of its normal value, mental

activity very soon becomes impaired and consciousness is lost. Undoubtedly the unraveling of the mechanism of associated memory is one of the greatest discoveries which biology has still in store.

VI.

ELEMENTARY PHYSIOLOGICAL PROCESSES.

It is, perhaps, possible that an advance in the analysis of the mechanism of memory will be made when we shall know more about the processes that occur in nerve cells in general. The most elementary mechanisms of self preservation in higher animals are the respiratory motions and the action of the heart. The impulse for the respiratory action starts from the nerve cells. As far as the impulses for the activity of the heart are concerned we can say that in one form at least they start from nerve cells, and in all cases from those regions where nerve cells are situated. But as far as the nature of these impulses is concerned we know as little about the cause of the rhythmical phenomena of respiration and heart beat as we know concerning the mechanism of associative memory. It is rather surprising, but nevertheless a fact, that physiology has not progressed beyond the stage of mere suggestions and hypotheses in the analysis of such elementary phenomena as nerve action, muscular contractility and cell division. Among the suggestions concerning the nature of contractility those seem most promising which take into consideration the phenomena of surface tension. The same lack of definite knowledge is found in regard to the changes in the sense organs which give rise to sensations. It is obvious that the most striking gaps in biology are found in that field of biology which has been cultivated by the physiologists. The reason for this is in part, that the analysis of the elementary protoplasmic processes is especially difficult, but I believe that there are other reasons. Medical physiologists have confined themselves to the study of a few organisms, and this has had the effect that for the last fifty years the same work has been repeated with slight modifications over and over again.

VII.

TECHNICAL BIOLOGY.

I think the creation of technical biology must be considered the most significant turn biology has taken during the last century. This turn is connected with a number of names, among which Liebig and Pasteur are the most prominent. Agriculture may be considered as an industry for the transformation of radiating into chemical energy. It was known for a long time that the green plants were able to build up, with the help of the light, the carbohydrates from the carbon dioxide of the air. Liebig showed that for the growth of the plant definite salts are necessary, that these salts are withdrawn from the soil by the plants, and that in order to produce crops these salts must be given back to the soil. One important point had not been cleared up by the work of Liebig, namely, the source of nitrates in the soil which the plants need for the manufacture of their proteins. This gap was filled by Hellriegel, who found that the tubercles of the leguminosæ, or rather the bacteria contained in these tubercles, are capable of transforming the inert nitrogen of the air into a form in which the plant can utilize it for the synthesis of its proteins. Winogradski subsequently discovered that not only the tubercle bacteria of leguminosæ are capable of fixing the nitrogen of the air in the soil in a form in which it can be utilized by the plant, but that the same can be done by certain other bacteria, for instance, *Chlostridium pasteurianum*. These facts have a bearing which goes beyond the interests of agriculture. The question of

obtaining nitrates from the nitrogen of the air is of importance also for chemical industry, and it is not impossible that chemists may one day utilize the experience obtained in nitrifying bacteria.

With the discovery of the culture of nitrifying bacteria we have already entered the field of Pasteur's work. Yeast had been used for the purposes of fermentation before Pasteur, but Pasteur freed this field of biology just as much from the influence of chance as Liebig did in the case of agriculture. The chemist Pasteur taught biologists how to discriminate between the useful and harmful forms of yeast and bacteria, and thus rendered it possible to put the industry of fermentation upon a safe basis.

In recent times the fact has often been mentioned that the coal fields will be exhausted sooner or later. If this is true every source of available energy which is neglected to-day may one day become of importance. Professor Hensen has recognized the importance of the surface of the ocean for the production of crops. The surface of the ocean is inhabited by endless masses of microscopic organisms which contain chlorophyl and which are capable of transforming the radiating energy of the sun into chemical energy.

Not only through the industry of fermentation and agriculture has technical biology asserted its place side by side with physical and chemical technology, but also in the conquest of new regions for civilization. As long as tropical countries are continually threatened by epidemics no steady industrial development is possible. Biology has begun to remove this danger. It is due to Koch if epidemics of cholera can be suppressed to-day and to Yersin if the spreading of plague can now be prevented. Theobald Smith discovered that the organisms of Texas fever are carried by a certain insect, and this discovery has had the effect of reducing and possibly in the near future destroying two dreaded diseases, namely, malaria and yellow fever.

It is natural that the rapid development of technical biology has reacted beneficially upon the development of theoretical biology. Just as physics and chemistry are receiving steadily new impulses from technology, the same is true for biology. The working out of the problems of immunity has created new fields for theoretical biology. Ehrlich has shown that in the case of immunity toxins are rendered harmless by their being bound by certain bodies, the so-called anti-toxins. The investigation of the nature and the origin of toxins in the case of acquired immunity is a new problem which technical biology has given to theoretical biology. The same may be said in regard to the experiments of Pfeifer and Bordet on bacteriolysis and hemolysis. Bordet's work has led to the development of methods which have been utilized for the determination of the blood relationship of animals.

VIII.

The representatives of the mental sciences often reproach the natural sciences that the latter only develop the material but not the mental or moral interests of humanity. It seems to me, however, that this statement is wrong. The struggle against superstition is entirely carried on by the natural sciences, and especially by the applied sciences. The nature of superstition consists in a gross misunderstanding of the causes of natural phenomena. I have not gained the impression that the mental sciences have been able to reduce the amount of superstition. Lourdes and Mecca are in no danger from the side of the representatives of the mental sciences, but only from the side of scientific medicine. Superstition disappears so slowly for the reason that the masses as a rule are not taught any sciences. If the day comes

when the chief laws of physics, chemistry and experimental biology are generally and adequately taught we may hope to see superstition and all its consequences disappear, but not before this.

As far as the influence of the applied sciences on ethics is concerned, I think we may hope that through the natural sciences the ethics of our political and economical life will be altered. In our political as well as our economical life we are still under the influence of the ancients, especially the Romans, who knew only one means of acquiring wealth, namely by dispossessing others of it. The natural sciences have shown that there is another and more effective way of acquiring wealth, namely, by creating it. The way of doing this consists in the invention of means by which the store of energy present in nature can be more fully utilized. The wealth of modern nations, of Germany and France, is not due to their statesmen or to their wars, but to the accomplishments of the scientists. It has been calculated that the inventions of Pasteur alone added a billion francs a year to the wealth of France. In the light of such facts it seems preposterous that statesmen should continue to instigate war simply for the conquest of territories. Through modern science the wealth of a nation can be increased much more quickly than through any territorial conquest. We can not expect any change in the political and economical ethics of nations until it is recognized that the lawmakers and statesmen must have a scientific training. If our lawmakers possessed such a training they would certainly not have allowed one general source of energy after another, such as oil fields, coal fields, water power, etc., to be appropriated by individuals. All these stores of energy belong just as well to the community as the oxygen of the air or the radiating energy of the sun. Our present economical and political ethics is still on the whole that of the classical period or the renaissance, because the knowledge of science among the masses and statesmen is still on that level, but the natural sciences will ultimately bring about as thorough a revolution in ethics as they have brought about in our material life.

IX.

If we compare the development of biology with the simultaneous development of physics and chemistry during the last twenty years, we must be impressed by the fact that during that time the great discoveries in physics and chemistry have followed each other surprisingly fast. The discovery of the law of osmotic pressure, the theory of electrical disassociation, the theory of galvanic batteries, the systematic formulation of physical chemistry, the discovery of electrical waves, the discovery of the X-rays, the discovery of the new elements in the air, the discovery of radioactivity, the transformation of radium into helium, the theory of radiation pressure—what have we in biology that could be compared with such a series of discoveries? But I believe that biology has important discoveries in store and that there is no intrinsic reason why it should be less fertile than physics and chemistry. I think the difference in the fertility of biology and the physical sciences is at least partly due to the present organization of the biological sciences.

General or experimental biology should be represented in our universities by special chairs and laboratories. It should be the task of this science to analyze and control those phenomena which are specifically characteristic of living organisms, namely, development, self preservation, and reproduction. The methods of general biology must be those of chemistry and especially those of physical chemistry. To-day general or experimental biology is represented

in our universities neither by chairs nor by laboratories. We have laboratories for physiology, but to show how little interest physiologists take in general biology I may mention the fact that the editor of a physiological annual review excludes papers on the development and fertilization from his report, as in his opinion, this belongs to anatomy. On the other hand, anatomists and zoologists must give their full energy to their morphological investigations and have, as a rule, neither the time for experimental work nor very often the training necessary for that kind of work. Only the botanists have kept up their interest in general biology, but they of course pay no attention to animal biology. In working out this short review of the development of biology during the last century I have been impressed with the necessity of our making better provisions for that side of biology where, in my opinion, the chances for the great discoveries seem to lie, namely, *general* or *experimental* biology.

JACQUES LOEB.

THE PROBLEMS OF EXPERIMENTAL PSYCHOLOGY.*

THE first difficulty that confronts one, as one attempts to envisage the problems of experimental psychology, is the difficulty of definition. What is a psychological experiment? What is the scope of experimental psychology? Is experiment simply a method of work, applicable to all or to some special parts of the psychological system; or is experimental psychology a distinct branch of psychology, sharply marked off from other and coordinate branches?

The program of this congress would seem to have decided the issue in the latter sense; for we find sections of general psychology, of comparative and genetic psychology, of abnormal psychology and of social psychology, arranged alongside of our own section of experimental psychology. If, then, I wished to take shelter behind the plan of the program, I might, with some show of justification, confine myself to the discussion of those problems in normal, human, adult psychology which still form the staple material of experimental investigation in the laboratories, and might omit all reference to the extensions of the experimental method to outlying fields. Such a course would, nevertheless, be unsatisfactory. The extensions of the method are coming to play a larger and larger part in psychological discussions and in our psychological literature; and it behooves us to take up a stand with regard to them, positive or negative, appreciative or critical. I shall try not to shirk this duty. Let me say, however, at the outset —and I shall have more to say upon the matter presently—that, whatever else experimental psychology may be, there can be no doubt that the subjects to which the program apparently limits us are experimental psychology. The examination, under strictly controlled and properly varied conditions, of the normal, adult, human mind—this is psychological experiment in its pure, primary and typical form. And it is this typical experimental psychology the problems of which we have, in the first place, to consider.

In approaching this question of the problems of experimental psychology, it seemed to me that the surest key to the future lay in the accomplishment of the past. The best way to find out what experimental psychology has to do is, I thought, to make certain of what it has already done. With this idea in mind, I naturally had recourse to our bibliographies—the American bibliography of the *Psychological Review,* and the German of the *Zeitschrift f. Psychologie.* The result was not encouraging. We

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all knew, of course, that the plan of arrangement of these two yearly lists is by no means the same. What I, for one, had not realized was the fact that the plan of arrangement of both is eminently unsystematic. We use a bibliography, and find it useful; we do not need to enquire further regarding it. But I do not believe that any psychologist, of whatever school, could write a systematic psychology on the lines laid down in these bibliographies. This fact—if fact it is—seems worthy of a passing remark; for it indicates, in a concrete and definite way, that in spite of the enormous increase of our psychological knowledge, within the last few decades, we are still very far from any complete or rounded science of psychology. I am not so much disposed to blame the bibliographers—I take their lack of system to be unavoidable—as I am to draw a long breath at the amount of work which still remains for us to do.

Finding that I could not avail myself of the bibliographies, I took the bull by the horns, and went to the psychological journals. I listed and analyzed the experimental papers in the *Philosophische Studien*, the *Zeitschrift f. Psychologie*, the *Année psychologique*, the *American Journal of Psychology* and the *Psychological Review;* not with any view of substituting a classification of my own for the classifications now employed, but simply with the intention of finding out what was there. If you object that these five journals are not coextensive with experimental psychology, I must reply that they are at any rate representative, and that the duration of human life is limited. Even so, I am not sure that the game was worth the candle. I earned, perhaps, by hard work, the right to stand upon this platform; but I found out very little that I did not know before.

If I am to indicate, briefly, the results of this enquiry, I must premise that we are agreed upon the distinction, within experimental psychology, between the properly 'psychological' and the psychophysical attitudes. The object of the 'psychological' experiment, as I am now using the phrase, is introspective acquaintance with the processes and formations of a given consciousness. The object of the psychophysical experiment, as we have recently been reminded by G. E. Müller—I suppose that we are all fresh from a reading of his 'Psychophysische Methodik'—is a numerical determination. Thus, the object of the simple reaction, regarded as a psychological experiment, is the introspective analysis of the action consciousness, given under certain fixed conditions; the object of the same experiment, regarded psychophysically, is the ascertainment of a representative time-value and of the manner and limits of its variation. Both points of view are covered by the general term 'experimental psychology'; both types of experiment are valuable; but the two must not be confused. If, now, we look at the contents of the *Philosophische Studien*, the oldest established of our five journals, we find that three departments of experimental investigation are preferred high above the rest: sensation, perception and action. There is, moreover, a very definite trend towards psychophysics, so that, *e. g.*, at least two fifths of the articles that deal with sensation must be classed outright as psychophysical. The remaining experimental papers may be subsumed under the headings: association of ideas, attention, feeling, memory and recognition, the organic accompaniments of the mental life, the range of consciousness, the processes involved in the activities of reading and writing, and the time consciousness. What we find in the other four journals is a continuance of interest in these same problems, but a continuance of interest which is combined with a shift of emphasis from psycho-

physics to psychology, and a widening of the area of experimental work. Thus in the *Studien* there are about twice as many articles on sensation, psychological and psychophysical, as there are on perception; in the *American Journal,* the articles on perception are more numerous than those on sensation; in the *Psychological Review* there are, roughly, three articles on perception for every two on sensation, while the strictly psychophysical papers may almost be counted upon the fingers of one hand; and the *Année psychologique,* if I have counted aright, has practically as many articles on memory as it has on perception, and more of either than it has on sensation, while the spirit of the work has, from the first, been adverse to psychophysics. Or again, the contents of the *American Journal* may, with some manipulation, be brought under the same headings that served for the *Studien,* save that one additional caption must be made for studies of voluntary movement (other than reactions) and of the experiences of effort and fatigue; while those of the *Zeitschrift* and the *Psychological Review* require at any rate three or four new rubrics, to cover work done upon mental inhibitions, the process of learning, motor automatisms and motor dispositions, habit, etc. I do not wish to labor this point, even if I must leave it with some sense of injustice to the periodicals under review. You know, without my telling you, and I knew, without going to the magazines, that the course of experimental psychology in recent years has been away from simple psychophysical determinations, and towards introspective analysis; and that the experimental method has been continually extended from the simpler processes to the more complex—whether to complexes hitherto untouched by experiment, or to unfamiliar phases of familiar mental formations. All that a study of the journals can do is to quantify and define these facts. I should like to add, however, that their study has brought home to me, in a very vivid way, the immense complexity and far-reaching interconnection of the mental life. The contents of experimental papers are oftentimes so varied that only a classification *a posteriori* is possible; and, oftentimes again, results that are but incidental to the given topic of investigation prove later on to be fundamental for problems from which this topic had seemed disconnected and remote.

So much, then, by way of preparation. Let us now, in the light of it, attempt to formulate the present problems of experimental psychology. You will remember that I am speaking of experimental psychology *sensu stricto*—of the experimental investigation of the normal, adult, human consciousness. I wish that I could proceed systematically. But, in the existing condition of the science, it is better to be topical. We may, however, begin in a quasi-systematic way, by considering the three fundamental problems of sensation, affection and attention.

(1) *Sensation.*—The senses, viewed from the standpoint of psychological knowledge, fall into three principal groups. We know a great deal about sight and hearing; we know a good deal about taste, smell and the cutaneous senses; of the organic sensations, with a very few exceptions, we know practically nothing. There is work to be done—I say this emphatically—in every field; there is probably no single chapter in sense psychology that may not, with advantage, be reopened. Nevertheless, we know a great deal about sight and hearing; the literature of these senses is voluminous; advance in our knowledge lies (I am speaking in the large and quite roughly) in the hands of the few experts who have occupied themselves particularly with visual and auditory problems. And we know a good deal about taste, smell and the cuta-

neous senses; although here, doubtless, there is much steady work, rank and file work, yet to be done. We know something of the organic complex concerned in active touch, and something of the static sense. On the other hand, of the organic sensations in general we know practically nothing. Here then, as I take it, lies the immediate sense problem for experimental psychology. When we remember the importance of organic sensation in the affective life, its importance as the vehicle of sensory judgments in psychophysical work, the part it plays in the mechanism of memory and recognition or in the motives to action, its importance for the primary perception of self; when we remember the widespread character of the organic reaction set up by any sensory stimulus; when we realize that some psychological systems have recourse to it from beginning to end, while others (Wundt's recent 'Grundzüge' is an example) practically ignore it; when we remember that certain questions of prime systematic importance hinge upon it—the question of the duality of the conscious elements, of the relative range of sensation and image, of what is called affective memory, and so on: we can hardly fail to see that here is a great gap in our psychological knowledge, the filling of which calls for a persistent application of the experimental method. Of all problems in the psychology of sense that are now before us, the problem of the number, nature and laws of connection of the organic sensations appears to me to be the most pressing.

In the domain of psychophysics, I see no single problem of supreme import, but rather a need for patient, continuous work by the methods already formulated. The inherent aim of psychophysical investigation is, as I have said, the determination of the psychophysical constants. Now it is by no means difficult to vary a psychophysical method, and so to set up a claim of originality; but it requires patience and some self-sacrifice to work through a psychophysical method to the bitter end. What we now want is less ingenuity and more work—accurate, continuous work all along the line. We have methods and we have formulæ. Let us give them a thorough test. The results will be of extreme value for psychophysics, and no one need fear that they will be barren for psychology. On the contrary, no small part of our analytical knowledge of the higher processes, as they are called—processes of judgment, of comparison, of abstraction—derives straight from the method-work of psychophysics. It would, in my opinion, be time and energy well spent, if every existing laboratory were to undertake what one might term the routine work of testing out, without modification, one or other of the classical methods.

I am aware that psychophysics trenches upon large problems. I ought, indeed, to be keenly alive to these problems, seeing that for the past three years they have occupied me, with but little intermission. There is the great problem of mental measurement itself; there are the minor problems of the validity of the difference limen, the equality of just noticeable differences, the range of Weber's Law, the correlation of functional constants, and what not. If I were speaking of the history of experimental psychology, and not of its present status, I might hope to show you that more has been done towards a solution of these problems than the current statements in text-books and magazines would lead one to suppose. But, with these problems in mind, I insist that the immediate demand in psychophysics is for careful, straightforward work by the approved methods. We shall gain more from such work than from anything else.

(2) *Affection.*—When we turn to the af-

fective processes, we have no such difficulty in selecting our problems. This whole chapter in experimental psychology is one single problem. Will you believe—I had myself not realized it before—that in all the five and thirty volumes of the *Zeitschrift* there is not a solitary experimental article on the feelings? This although the same volumes contain, roughly, two hundred contributions to experimental psychology! The *Studien* has about one hundred and forty experimental papers, of which nine deal with affective psychology or experimental æsthetics: that is the best record I have found. Now look at the problems. We are not at one as regards the nature and number of the elementary affections; there are experimental psychologists who reduce all the elements of consciousness to sensations. We are not agreed whether the diversity of feelings is to be referred to a diversity of affective process proper or to a diversity of organic sensation. Some of us think that a given affective process is coextensive with consciousness; others maintain that consciousness may be a mosaic of affections. Some assert that the feeling element is effective for association; others deny it this effectiveness. Some find the best illustrations of the law of contrast in the sphere of feeling; to others, contrast may itself be a feeling. Our facts are few, our laws dubious. Surely, it is time to gird up our loins and make serious business of these affective problems.

I have insisted on the paucity of the experimental articles upon feeling. I do not, by this, mean to accuse experimental psychology of idleness or neglect: Lehmann's two books would save us from such a charge, if we had nothing else to offer. But these two books are characterized by their reliance upon the expressive method—a method which, as you are aware, has stood in the forefront of many recent discussions. I have been at the pains to make out a complete table—complete, that is, so far as I was able to make it complete—of the results obtained by the method of expression. There is much to be learned from them. But I can not believe that the method will help us very greatly towards an affective psychology. The organic reactions which the expressive method registers are closely interwoven and interdependent, and the task of differentiation presents difficulties which, if not insurmountable, have at least not yet been surmounted. I am disposed to think, *e. g.*, that the plethysmograph, as a differential instrument, is doomed to disappear from our laboratories. The sphygmograph, and especially the pneumograph, hold out better hope; but I doubt if, at the best, a differentiation of affective qualities is to be expected from them. From the method of suggestion, which really takes us over into social psychology, I expect still less. There remains, at present, only the method of impression, which has done good service in a limited field, and which should be capable of modification and expansion. However, I am fortunately not called upon here to propose methods of work, but only to indicate problems. And the facts and laws of the affective life, the life of feeling and emotion, form one of the largest and one of the most insistent problems of modern experimental psychology.

(3) *Attention.*—The prominence given to the state of attention is characteristic of experimental psychology, as contrasted with the empirical psychology of associationism. It is, indeed, one of Wundt's greatest services to the new psychology that he early divined the cardinal importance of attention in the psychological system, and began that series of experiments of which we can by no means see the end to-day. For I imagine that we must all admit, if we are honest with ourselves, that the body of

facts at our disposal, large and varied as it is, is yet not adequate to a theory of the attentive state. We must know more of the constitution of the attentive consciousness, and of the mechanism of distraction; much remains to be done before we can settle the vexed questions of the distribution of attention; we must work out, experimentally, the relation of attention to affective process; even the familiar problems of the range and duration of the attentive state are—well, are still problems. I am not sure that we shall not have to manifold the study of attention, as we have that of memory; and to speak in future of the facts and laws of visual attention, auditory attention, and so on, instead of taking 'attention' as a single state. I am certain that we must have a more specialized psychology of the great variants and resultants of attention—a specialized psychology of expectation and habituation, of practise and fatigue.

If, then, I have seized the situation correctly, we have in these three fundamental departments of psychology three problems of different orders, the solution of which calls for a diverse endowment of psychological skill and insight. There is an outlying group of sensations that can, we must believe, be successfully attacked by the analytic methods which have been successfully employed in the other sense departments. The experimental study of the affective processes calls for a much greater gift of originality and constructive imagination; we have to shake off literature and tradition, and to begin almost at the beginning. In the case of attention, we have to push on and make progress along paths already marked out but insufficiently explored.

What holds in this regard of the attention seems to me to hold also (4) for that mixed medley of formations which we include under the general term *perception.* I wish that we could banish the word 'perception' to the special limbo reserved for unregenerate concepts, and could put in its place a round dozen of concrete and descriptive terms! But it has, so far, held its own, and I can hardly avoid its use. We know, now, a great deal about tonal fusion, about space perception, about rhythm—if rhythm be a perception; we know something about time perception. You will, however, agree with me that no one of these topics is a closed chapter. I see no very pressing problem, as I look over the field; but I see, in every quarter of it, good work that needs doing. I am sorry if this opinion appears indefinite; it is the opinion that I have come to after a study of more than a hundred and fifty articles that deal with perception in the five journals referred to just now: and I can not make it more definite without going so deeply into detail as far to exceed the time allotted to me.

We can speak a little more concretely of (5) *recognition, memory* and *association.* Association was, at first, handled in rather stepmotherly fashion by experimental psychology. Of late years, however, we have come to see the importance of detailed analyses of the associative, as also of the recognitive consciousness; we have, I think, finally broken free from the traditional schemata, and are approaching the problem with open minds. Something has already been done; much more remains to do. The experimental study of memory was begun, by Ebbinghaus, rather in a practical or psychophysical than in a psychological spirit. In the development of the work since Ebbinghaus, we can trace two tendencies: a tendency towards psychological analysis of the memory consciousness and the explication of the psychological laws of memory: that on the one hand; and on the other, a tendency towards the application in practise of psy-

chological results. While, now, I take the recent experimental work on memory and the associations involved in memory to be work of a high order; and while I believe, in particular, that certain of the methods employed are a valuable addition to our psychological repertory, I can not but think that the two tendencies just mentioned have not been kept as distinct as they should have been, and that experimental psychology has suffered in consequence. We can hardly hope to get a psychology of memory and association on the ground of Reproduktionstendenz and Perseverationstendenz: we can hardly hope to get practical rules, if they are what we want, out of the published studies on economy of learning. The Tendenz-concepts are psychophysical, and tend to cover up the complexity of actual experience; the practical studies are made under conditions widely remote from those that obtain in ordinary practise. Let us realize that we may attempt here any one of three distinct problems. We may aim at a psychology of memory and association; *i. e.*, we may seek to record our experience, to trace the introspective patterning of the memory consciousness. We may aim at a psychophysics of memory; *i. e.*, we may try to establish formulæ akin to the well-known formula of Ebbinghaus' 'Gedächtnis,' which represents retention as a function of time elapsed. Or we may aim at an applied psychology of memory; we may work out, experimentally, an art of acquisition. I do not say that an investigation into one of these three topics will throw no light on the other two; on the contrary, I have already insisted on the value of indirect results in psychological enquiries. But in our thought, at any rate, the three problems should remain separate and distinct. They offer, without doubt, a wide field for future research. I would suggest, though with all reserve, that the psychological study of memory and association may, in the long run, help us to clear up the much-disputed question of the subconscious. There are, as you know, experimental psychologists who work simply in terms of introspection and of physiological process; there are others who interpolate between these terms an unconscious or subconscious mentality. I can not go into detail; but it seems to me that, if these differences of opinion can in any connection be brought into the laboratory for adjustment, it is here, in the investigation of memory and association, that we may hope to introduce them.

I come next (6) to *action*. You will remember that, in its early years, experimental psychology was much concerned with the psychophysics of action; indeed, the problem of the 'personal equation' is a good deal older than our laboratories. This interest has never flagged. If we have not heard so much of late about reaction experiments, we have heard a great deal about the psychophysiology and psychophysics of voluntary movement. And I think that we can leave those things to take care of themselves; we may, without any question, look to the next few years for improvements of technique, for revision of numerical determinations, for recasting of theories. That work is under way. What I should like now to emphasize is the need for investigation of the more strictly psychological kind. Our knowledge of the action consciousness is still very schematic, very rough, in part very hypothetical. It has been recognized for some years that the reaction experiment may be turned to qualitative, *i. e.*, to analytical account; but so far more use has been made of this idea in laboratory practise than in research. We must start all over again, and take the action consciousness seriously. I once made a sort of reaction experiment of the setting-up and

taking-down of an inductorium; the student made the manipulations continuously, under time control, and gave his introspective record at the end of each experiment. We worked at the problem for a year, only to learn that we had been too ambitious; we had, as even with experience one is apt to do, underestimated the complexity of consciousness. At the same time, we decided that the problem was soluble; we gathered in a good store of introspective results, even if they were too individual, and too discrete, to be employed for generalization; with more time and more observers, or with a simpler set of voluntary movements for study, we should have accomplished something for psychology. I regard such studies as those recently made on the control of the retrahens of the ear, or on the control of the winking reflex, as extremely promising in this field. At any rate, whether we work from the classical reaction experiment, or whether we take voluntary movement under more natural conditions, the problem is quite definite: we must submit action to an introspective analysis as detailed and as searching as that to which we have subjected perception.

I have put off (7) *imagination*, because I am a little afraid of the term. It is a word which, like perception, I should be glad to see discarded from the vocabulary of experimental psychology. I think that we employ it more vaguely even than we employ perception; and I think that the future will substitute for it a number of descriptive terms. If we begin with the elementary process, the image itself, we must plead ignorance on two fundamental points: whether image quality is coextensive with sensation quality, and whether image difference is adequate to sense discrimination. If we go to the other extreme, and regard imagination as the general name for a group of typical formations—as a concept coordinate with memory—we must surely say that experimental psychology is, as yet, hardly over the threshold of the subject. We know, perhaps, how to set to work: some investigations have been made, and some hints toward method have been given; but, in the large, this chapter of experimental psychology remains to be written.

(8) Of the more complex *affective* formations we can say but little until we have a better psychology of feeling. No doubt, there are certain problems in the psychology of sentiment, and more especially in that of the esthetic sentiments, that can, within limits, be handled without regard to the ultimate categories of feeling. I should, however, consider these limits as very strictly drawn. (9) For the higher *intellectual* processes we have, I think, three sources of knowledge: direct experiment—that, as you know, has been well begun,—the indirect results of experiment upon sensation, and Völkerpsychologie. I am inclined to lay great stress upon the second of these sources. Experimental psychology has often been reproached, on the one hand, because it devotes most of its time to sensation, and on the other because the results of its dealings with the higher processes are jejune and meager. To the former charge I plead guilty, in so far as we have avoided the affective problems, though this neglect is not at all what the framers of the accusation have in mind. And even so, I might offer in extenuation the experimental work upon attention. But this apart, I think that experimental psychology is justified in its choice of topics. The only way to catch the higher intellectual processes in course of formation is to work from the periphery, by way of the sense organs. It is when we are working with tones, or with lifted weights, that the amazing diversity and complexity of judgment becomes apparent. If, on the con-

trary, we take any one of these higher processes full-formed, and attack it directly, we are very likely to find that the vehicle of the mental function is extremely simple; there is a law of reduction, running all through mind, whereby a highly complex formation tends to degenerate, to reduce to a stereotyped simplicity. It is, to my mind, a distinct merit of experimental psychology that it has brought to light this meagerness of content in the examination of 'higher' mental functions of an habitual order; and it is a healthy instinct that sends us back and back again to the channels of sense, as we seek an appreciation of the fulness and richness of the mental life. I may add, though I say this a little hesitatingly, as a merely personal impression, that the introspective attitude of the observer seems to me to be more nearly normal, less artificial, in cases where the avowed object of experimentation is comparatively simple. If you are asked overtly to grapple with a complex psychosis, you are likely to brace yourself to the task, to put on an armor of preconceived opinion; if the psychosis meets you unawares, finds you off guard, the facts will have their own way with you. A distinguished English psychologist once declared that it is futile to attempt the problems of recognition by way of rotating discs of black and white sectors. I should say, on the contrary, that these discs are, in principle, the very best means to an understanding of the higher intellectual formations.

As for the ultimate goal of experimental endeavor, I suppose that we may call it (10) the problem of *consciousness*,—not in the sense in which that problem is understood by the theorist of knowledge, but in this sense: that, as hitherto we have analyzed and traced to their conditions certain mental processes, of lesser or higher degrees of complication, so now we analyze and trace to their conditions total consciousness, given in varying states and constituted of various formations. The difficulty of this problem is enormous. Only those of you who have attempted it, in one case or other, for yourselves, who have discarded classificatory terms, and faced the living facts; only these, even of experimental psychologists by profession and training, can form any proper idea of its difficulty. It is a problem for which we are not yet ripe. We can approach it only by way of theories which we know to be inadequate, and by help of hypotheses which we can not substantiate by facts. But it is the problem towards which we are trending, and the road to its solution lies, as in my judgment all such roads in our science lie, not through brilliant suggestion and ingenious forecast, but through patient and steady work. This work must be in part the work of experimental psychology, as we are here interpreting that phrase; in part the work of what is called individual psychology—though, indeed, from perception onwards, the difference between these two departments of psychological investigation is simply a difference of accent. Or, to put the matter concretely, we must work not only with the doctrine of states of consciousness, comparing experimentally the attentive and the inattentive, the hypnotic and the dreaming, all sorts of normal and abnormal states of consciousness, but also with the doctrine of conscious types which we owe (and the debt is great) to the psychologists of individual variation.

So I finish the first part of my review. If I have omitted anything of consequence, or if I have seemed to do injustice to any department of work, I must ask for pardon and correction; I have spoken with the utmost possible brevity. My own habitual thought in experimental psychology is positive, not negative; that is, I am accustomed to look upon our problems rather as con-

tinuations of work already begun than as gaps and lacunæ in our system of knowledge. I could wish that it had fallen to my lot to address you in this positive way, to show what experimental psychology has done, how in the past few decades it has changed the face of systematic psychology, rather than to insist upon the tasks that still lie before it. I have, however, tried to be entirely honest; I have, I think, rather exaggerated than concealed our deficiencies; and I would have you remember that this definite formulation of things to do presupposes and implies that much has been done. When Wundt wrote his famous essay 'Ueber die Aufgaben der experimentellen Psychologie,' the problems that loomed before him were the psychophysics of sensation, the analysis of perception, the time-relations of the higher processes. To-day, the list is longer and the range wider. But it is only because we already possess that we can say, in such detail, what still needs to be added to our possessions: in which fact let us take encouragement.

I pass, with some diffidence, to a consideration of wider issues—of those extensions of the experimental method, proposed or attempted, of which I spoke at the beginning of this address. Most psychologists, I take it, would agree that the picture I have drawn of experimental psychology in what has preceded is drawn too narrowly. The title of psychologist is, indeed, given at the present day to two distinct types of scholar. On the one hand, we have the psychologist as I have represented him: a man keenly interested in mind, with no purpose beyond mind; a man enamored of introspection; a man to whom the most fascinating thing in the universe is the human consciousness; a man to whom successful analysis of an unresolved mental complex is as the discovery of a new genus to the zoologist or a new river to the explorer; a man who lives in direct companionship with his mental processes as the naturalist lives with the creatures that are ordinarily shunned or ignored; a man to whom the facts and laws of mind are, if I may so put it, the most real things that the world can show. On the other hand, we have men to whom mind appeals either as a datum or problem, or both, to be dealt with by philosophy, by theory of knowledge and theory of being; or as a natural phenomenon, something that must be taken account of whenever life is taken account of, in evolutionary biology, in anthropology, in medicine, and where not. Of the psychologists of this second order, the philosophers, you will say, do not concern us. Yet they do, somewhat. I suppose that all sciences—certainly, all young sciences—are liable to be told by well-wishers that they have mistaken their work; that they would advance more quickly, and more solidly, if they would put off their present business, and settle down to this or that suggested problem. At any rate, experimental psychology has always received such hortation from friendly philosophers. If, now, I have ignored this advice, it is not from lack of gratitude, but simply because, after consideration, I have come to believe that experimental psychology knows what she is about, and can walk without assistance. Outsiders, we are told, see most of the game. I venture to urge that the insider better knows how the game is to be played.

We are left with the two opposed types: what shall I term them?—the inner and the outer, the subjective and the objective, the narrower and the broader. What, then, of the outer, wider, objective problems of experimental psychology?

Let us be clear, first of all—the matter admits of no hesitation or compromise—that the experimental psychology of the normal, adult, human mind must take the

form that I have described—the form of introspective analysis. I have little sympathy or patience with those experimentalists who would build up an experimental psychology out of psychophysics and logic; who throw stimuli into the organism, take reactions out, and then, from some change in the nature of the reactions, *infer* the fact of a change in consciousness. Why in the world should one argue and infer, when consciousness itself is there, always there, waiting to be interrogated? This is but a penny-in-the-slot sort of science. Compared with introspective psychology, it is quick, it is easy, it is often showy. We have been a little bit corrupted by the early interest in psychophysics; or perhaps, more truly, we have not all learned instinctively to distinguish between psychophysics and psychology proper; and so we are apt to take the tables and curves of reactions for psychological results, and the inferences from them for psychological laws. Now the results, where they are not purely physiological or anthropometrical, are psychophysical results. As such, they have their usefulness; and the psychological laboratory is their right place of origin. But there is no reason why one should gain psychological credit for them —still less for erecting a speculative psychology upon their foundation. This mode of psychologizing is inherently as vicious as any of the constructive modes of the older psychology, the psychology before experiment. Historically, it has proved disastrous;* it falsifies problems and obscures real issues; we must set our faces against it now and for all time. How, indeed, shall one call a man a psychologist who deliberately turns his back upon the one psychological method, in the one field to which that method directly applies? There is no excuse, in psychology, for the neglect of introspection, save the one—and that must be demonstrated—that introspection is impossible.

* Is proof needed? Think of the early work upon the just noticeable difference, upon the simple reaction, upon the 'time sense'; or think of Wundt's current discussion of Weber's and Merkel's laws!

Having said this much by way of preface, I may take up the further question. We can hardly open a magazine nowadays without finding applications of the experimental method beyond the limits of the normal, adult, human mind. In animal psychology, in child psychology, in various departments of mental pathology, the experimental method is employed. Even the conservative *Studien* contains articles on the state of sleep and dreaming, and Wundt has looked more favorably upon experiments under hypnosis since they promise to confirm his theory of feeling. Experiments on children and animals have for some years past occupied the attention of leading American psychologists; work on child psychology is characteristic of the *Année psychologique,* and is being published more and more freely by the *Zeitschrift;* you all know the avowed purpose of Kraepelin's 'Arbeiten.' I need not multiply references. Wherever psychological interest has gone, in these fields, the experimental method has gone with it. Sometimes the particular experiment is borrowed forthright from the normal practise of the laboratory, sometimes the procedure has been recast to suit the novel problem; sometimes the experimental method is taken seriously, employed with care and knowledge, sometimes it is thrown in as a makeweight, without responsibility or understanding; sometimes it is praised, sometimes decried. All this is natural. The important thing for us is, I think, the recognition that the experiments are a part of 'experimental psychology,' in the sense of this paper, and must be taken account of in any general review

of the problems of experimental psychology. The psychologist of the laboratory is apt to emphasize the crudity and roughness of the work, and its neglect of introspective control; the psychologist of the clinic or the schoolroom or the animal room is apt to consider his colleague narrow and his colleague's work finical and meticulous. The transcending of this difference, the reconciliation of these views, I take to be a very real problem for experimental psychology—though a problem of a different order from those that I have been discussing. And I suggest the following points for your consideration. First, one can not be too nice or too careful in experimenting on mind. There is no such thing as over-refinement of method.* Let those who doubt this fact read Martin and Müller's 'Unterschiedsempfindlichkeit'; the more delicately one analyzes, the more subtle does mental process reveal itself to be. Galton's questionary results on visualization are psychology, and valuable psychology; but they are also pioneer psychology. Now, the pioneer may pride himself on his work, but not on the roughness of his work. When the laboratory psychologist smiles at the charcoal sketches of objective experiment—well, he does wrong to smile, because honest work should not be laughed at; but he is right in his conviction that the details are all to come, and that the simplification of the lines means over-hasty generalization. Mind is, so to say, our common enemy; and the laboratory psychologist learns, by dearly bought experience, not to underestimate his opponent. Secondly, I would remind you that, after all, objective work in psychology must always be inferential; introspection gives the pattern, sets the standard, of analysis and explanation. If we interpret the animal mind by the law of parsimony, our only justification is that introspection discovers the reign of this law in the human consciousness; if we subsume the evolution of mind in the animal series to the principle of natural selection, our only justification is, again, that introspection discovers the working of this same principle in our own case. As I put it just now, there is but one excuse for the neglect of introspection in psychology, and that is that introspection is impossible; but even here our neglect is methodical only, and does not—must not—extend to interpretation. These things have been said so often* that they have become commonplaces; but even a commonplace may be true—and it makes a difference, too, whether the truth be urged with polemical or with friendly intent. I should like to see more cooperation between the alienist, or the student of comparative psychology, and the laboratory psychologist; quite apart from practical results, such cooperation would be of great advantage to the psychological system. We can hardly hope —this point should be borne in mind—that the two interests, the objective and the subjective, will be combined in the same person. When one has once stepped inside the ring of the normal, adult consciousness, there is very little temptation to step out again; the problems that I listed a little while ago are enough to occupy several generations of workers, and the fascination of the work is like the fascination of the mountains or the sea. And if one begins from the outside, with the child or the animal or the abnormal mind, there is little

* A method may be too refined for the man who is using it, or for the problem upon which he is immediately engaged. But these are different matters.

* In saying them, from the 'narrower' point of view, I am, of course, hoping for similar cautions (at any rate, for varied advice and information) from the more 'objective' psychologists. What they will have to tell their colleagues of the laboratory, I do not know; but I have no doubt that it will be worth listening to.

likelihood that one can breathe the confining air of the laboratory, or that one will presently limit one's range of interests to oneself. Partly it is a matter of temperament, partly a matter of chance introduction or of continued occupation. The two types of psychologist are distinct: all the more reason that they should work in harmonious cooperation.

I hope that, in this latter portion of my address, I have not traveled too far out of the record. Some men have problems thrust upon them. And, after all, if what I have said contributes ever so little to the furtherance of mutual aid and the increase of mutual esteem, as between psychologists of different camps, I may hope for forgiveness, even though I have exceeded the letter of my instructions. Now let me briefly summarize what I have said. I began, you will remember, by pointing out that, above and apart from the many special problems of experimental psychology, there lies the great problem of self-definition, of the range and scope of the experimental method in psychology. Then, under the headings of psychology proper and of psychophysics, I called your attention to a series of laboratory problems that, more or less insistently, more or less immediately, call for solution. Whatever else experimental psychology may be, I said, these issues are issues of experimental psychology. Incidentally, I deprecated any departure, at the bidding of philosophy, from the straight path of psychological investigation; and I deprecated also that neglect of introspective control in psychology which has been the besetting sin of many whose direct interest lies in psychophysics. I then went on to include in experimental psychology the more objective applications of the experimental method in child psychology, in animal psychology, in abnormal psychology. It was not my province to detail the special questions in these fields; they form the topic of other addresses in other sections. But I should regard as incomplete any review of the problems of experimental psychology which omitted reference to them. Their consideration helps us to attack that first problem of definition, clarifies our method, and furnishes an opportunity for the give-and-take of criticism and encouragement. We can not afford to misunderstand one another, as we can not afford to waste our time on unreal and constructive problems. The work presses; the rule of work is definite and unmistakable; there is room in the workshop for all sorts and conditions of men. I do not think that the outlook of any science could be more hopeful; I do not think that we need fear a lessening of that quiet enthusiasm which, from the first, in the beginner as in the mature student, has been the salient characteristic of the experimental psychologist.

E. B. TITCHENER.

SCIENTIFIC BOOKS.

An Introduction to the Theory of Mental and Social Measurements. By EDWARD L. THORNDIKE. New York, The Science Press. 1904. Pp. 212. 8vo. Price, $1.50.

In this book Dr. Thorndike has undertaken to explain the 'meaning and use' of recent contributions to statistical theory 'in common language to a common-sense thinker.' "Knowledge will be presupposed of only the elements of arithmetic and algebra. Artificial symbols will be used only where they are really convenient." In order are discussed: Units of measurement, the measurement of an individual and a group, the causes of variability and the theory of probability, the arithmetic of calculating central tendencies and variabilities, the transmutation of measures by relative position into terms of units of amount, the measurement of differences, changes and relationships and the use of tables, reliability of measures and errors of measurements.

On the whole, the author's aim seems to be realized, although it takes over 200 pages. The frequency polygon, as a whole, is properly declared to be the unit of comparison which its constants by no means fully replace. The methods of determining the average, standard derivation and probable error are fully set forth and the explanation of the method of calculating the coefficient of correlation is particularly good.

Great stress is laid—properly enough in a book intended for psychologists whose material is not always directly measurable—on measurement by position and the transmutation of position into units of amount. Such a transmutation is easily effected when the frequency distribution is approximately normal. A little table, based on the table of the normal probability integral, is given, showing the deviation from the mean (in units of the standard deviation) of each per cent. class from 1 to 50. A handy table is also given showing the average deviation of any number of consecutive percentage classes. Of course, there is nothing new in this, but it helps to have the importance of the measure by relative position insisted on in a popular treatise of this sort, because it is not popularly understood.

In treating the measure of differences emphasis is laid on the importance of comparing the entire distributions rather than the averages only. The degree of overlapping of the frequency polygons gives the best insight into the degree of difference.

Under 'Measurement of Relationships' the measurement of correlation is considered and the Pearsonian method of analysis is plainly and fully set forth. In the chapter on 'Reliability of Measures' the determination of the probable error of the average and of a difference between two averages is fully described.

The book abounds in tables giving various statistical data. There is appended a multiplication table up to 100×100; also a table of squares and square roots. A table of the normal probability integral (apparently copied without credit from the reviewer's 'Statistical Methods') is found on page 148. A feature of the work is a set of 'Problems' at the end of each chapter.

The reviewer has noted in passing several defects which are mentioned here in order that they may be guarded against in the second edition. Part of Fig. 12 seems to be inverted. The 'Mode' is repeatedly spoken of where empirical and not theoretical mode is meant. The distinction should always be clearly made. Also, the mode is not the 'apex of the slope' (p. 73), but the abscissa of the apex. The method suggested of finding the mode is unnecessarily clumsy. The mode is approximately equal to the mean less $3 \times$ (mean — median). Tables XXXI. and XXXII., the first value of σ would seem to be a misprint for 2.57.

On the whole, we believe the book will be found very useful, especially in making more familiar the frequency polygon and leading to its more frequent publication in statistics in place of the bare average. And so we trust that it will be widely studied and its recommendations followed. C. B. DAVENPORT.

American Hydroids. Part II. *The Sertularidæ.* With 41 plates. By C. C. NUTTING. Special Bulletin, U. S. National Museum. 1904.

The first part of this magnificent work, on the Plumularidæ, appeared in 1900, and was noticed at some length in our columns. Much of what was said about Part I. is equally applicable to Part II., and need not be repeated. Some idea of the value of the work may be gained from the fact that not more than 20 species of Sertularidæ from American waters have heretofore been discussed in any single publication, and now Professor Nutting presents us with complete descriptions and figures of no less than 130! These species, distributed by the author in ten genera, have been named by the following writers: Nutting, 37; Allman, 16; Linnæus, 12; S. F. Clark, 9; Kirchenpauer, 8; Hartlaub and Mereschkowsky, each 5; Ellis and Solander, Hincks, Trask, d'Orbigny and H. B. Torrey, each 3; Levinsen, Alder, Bale, Marktanner-Turneretscher, Murray and Lepechin, each 2; and J. E. Gray, McCready, Versluys, Poeppig, Stimpson, Sars,

Lamouroux, Meyen, Fewkes, Johnston, Busk and Verrill, one each. These figures show that although many able zoologists have studied these animals, Nutting has done far more to make known the American species than any of his predecessors.

In a few instances I find myself unable to agree with Professor Nutting's nomenclature, if I rightly understand the facts. Thus *Thuiaria dalli* is a new name for *Sertularia cupressoides*, Clark, 1876, because it is a *Thuiaria*, and conflicts with *T. cupressoides*, Kirchenpauer, 1884. The rule here followed is that recognized by many botanists, but is, I think, losing ground, while it is not usually considered valid in zoology. Surely it would be more in accordance with zoological custom (and, I think, common sense) to give priority to the older specific name, no matter what genus it was placed in, and consequently rename the species of Kirchenpauer, not that of Clark.

The natural history department of the British Museum is constantly referred to as the 'South Kensington Museum,' which is not exact, and would be understood by Londoners to refer to a different institution.

T. D. A. Cockerell.

SOCIETIES AND ACADEMIES.

THE CONVOCATION WEEK MEETINGS OF SCIENTIFIC SOCIETIES.

The American Association for the Advancement of Science, the American Society of Naturalists and the following societies will meet at Philadelphia, Pa., during the week beginning December 24, 1904:

The American Association for the Advancement of Science.—The week beginning on December 27, President, Professor W. G. Farlow; permanent secretary, Dr. L. O. Howard, Cosmos Club, Washington, D. C.; general secretary, President Charles S. Howe, Case School, Cleveland, Ohio; secretary of the council, Professor Clarence A. Waldo, Purdue University, Lafayette, Ind.

Local Executive Committee.—President, Provost Charles C. Harrison; vice-president, Professor Edgar F. Smith; secretary, Dr. Philip P. Calvert; treasurer, Dr. Samuel G. Dixon; chairman of the executive committee, Provost Charles C. Harrison; of the committee on reception and entertainment, Mrs. Charles C. Harrison; of the committee on hotels and boarding houses, Professor Amos P. Brown; of the committee on meeting places and equipment, Professor Edwin G. Conklin; of the committee on press and printing, Mr. George E. Nitzsche; of the committee on transportation, Mr. Walter Wood; of the committee on finance, Mr. S. F. Houston.

Section A, Mathematics and Astronomy.—Vice-president, Professor Alexander Ziwet, University of Michigan; Secretary, Professor L. G. Weld, University of Iowa, Iowa City, Iowa.

Section B, Physics.—Vice-president, Professor Wm. F. Magie, Princeton University; Secretary, Professor Dayton C. Miller, Case School of Applied Science, Cleveland, Ohio.

Section C, Chemistry.—Vice-president, Professor Leonard P. Kinnicutt, Polytechnic Institute, Worcester, Mass.; secretary, Professor Charles L. Parsons, New Hampshire College of Agriculture, Durham, N. H.

Section D, Mechanical Science and Engineering.—Vice-president, Professor David S. Jacobus, Stevens Institute, Hoboken, N. J.; secretary, Professor Wm. T. Magruder, Ohio State University, Columbus, Ohio.

Section E, Geology and Geography.—Vice-president, Professor Eugene A. Smith, University of Alabama; secretary, Dr. Edmund O. Hovey, American Museum of Natural History, New York, N. Y.

Section F, Zoology.—Vice-president, Dr. C. Hart Merriam, U. S. Dept. of Agriculture; secretary, Professor C. Judson Herrick, Denison University, Granville, Ohio.

Section G, Botany.—Vice-president, Professor B. L. Robinson, Harvard University; Secretary, Professor F. E. Lloyd, Teachers College, Columbia University, New York, N. Y.

Section H, Anthropology.—Vice-president, Dr. Walter Hough, U. S. National Museum; secretary, George H. Pepper, American Museum of Natural History.

Section I, Social and Economic Science.—Vice-president, Martin A. Knapp, U. S. Interstate Commerce Commission, Washington; Secretary, Dr. J. F. Crowell, Bureau of Statistics, Washington, D. C.

Section K, Physiology and Experimental Medicine.—Vice-president, Professor H. P. Bowditch, Harvard University.

The American Society of Naturalists.—December 27, 28. President, Professor E. L. Mark, Harvard University; secretary, Dr. Chas. B. Daven-

port, Station for Experimental Evolution, Cold Spring Harbor, Long Island, N. Y.

The Astronomical and Astrophysical Society of America.—December 28, 29. President, Professor Simon Newcomb; secretary, Professor Geo. C. Comstock, Washburn Observatory, Madison, Wis.

The American Physical Society.—December 30. President, Professor Arthur G. Webster; secretary, Professor Ernest Merritt, Cornell University, Ithaca, N. Y.

The American Chemical Society.—December 28–31. President, Professor Arthur A. Noyes, Massachusetts Institute of Technology; Secretary, Professor William A. Noyes, the Bureau of Standards, Washington, D. C.

The Geological Society of America.—December 29–31. President, Professor J. C. Branner, Stanford University; secretary, Professor Herman L. Fairchild, Rochester, N. Y.

The Botanical Society of America.—December 27–31. President, F. V. Coville; secretary, Dr. D. T. MacDougal, N. Y. Botanical Garden, Bronx Park, New York City.

The Society for Plant Morphology and Physiology.—December 28, 29, 30. President, Dr. G. T. Moore, Department of Agriculture, Washington; secretary, Professor W. F. Ganong, Smith College, Northampton, Mass.

The Botanical Club of the Association.

The Fern Chapter.

Sullivant Moss Chapter.

Wild Flower Preservation Society of America.

The Society for Horticultural Science.—President, Professor L. H. Bailey, Cornell University; secretary, S. A. Beach, Geneva, N. Y.

The Society for the Promotion of Agricultural Science.—December 26. Secretary, Professor F. M. Webster, University of Illinois, Urbana, Ill.

The Association of Plant and Animal Breeders.

The Association of Economic Entomologists.—President, Professor A. L. Quaintance, Washington, D. C.; secretary, Professor H. E. Summers, Ames, Iowa.

The Entomological Club of the Association.

The American Society of Zoologists (Eastern Branch).—December 27, 28. President, Professor E. A. Andrews, Johns Hopkins University; secretary, Professor Gilman A. Drew, University of Maine.

The American Society of Vertebrate Paleontologists.—December 28–30. President, Professor H. F. Osborn, Columbia University; secretary, Dr. O. P. Hay, American Museum of Natural History, New York City.

The Society of American Bacteriologists.—President, Professor F. G. Novy, University of Michigan; secretary, Professor F. P. Gorham, Brown University, Providence, R. I.

The American Physiological Society.—December 27, 28. President, Professor R. H. Chittenden, Yale University; secretary, Professor Lafayette B. Mendel, New Haven.

The Association of American Anatomists. December 26, 27, 28. President, Professor Charles S. Minot, Harvard Medical School; secretary, Professor G. Carl Huber, 333 East Ann St., Ann Arbor, Mich.

American Folk-Lore Society.

The American Anthropological Association.—December 27–Jan. 2. President, Dr. W J McGee, Washington; secretary, Dr. Geo. Grant MacCurdy, Yale University, New Haven, Conn.

The American Psychological Association.—December 28, 29. President, Professor William James, Harvard University; secretary, Professor Livingston Farrand, Columbia University, New York City.

The American Philosophical Association.—December 28, 29, 30. President, Professor George T. Ladd, Yale University; secretary, Professor H. N. Gardiner, Northampton, Mass.

The Sigma Xi Honorary Scientific Society.—President, Professor S. W. Williston, University of Chicago; secretary, Professor Edwin S. Crawley, University of Pennsylvania, Philadelphia, Pa.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

THE 590th regular meeting was held November 12, President Marvin in the chair.

Mr. Marsden Manson, of San Francisco, presented by invitation a paper on 'The Evolution of Climate.' In opposition to the modern views which attribute geological climate to solar control and the glacial epoch to astronomical causes, the author emphasizes the influence of the dense aqueous atmosphere which must have surrounded the earth in early times, and the change that occurred when the sun's rays could reach the surface. The ice-age was a transition period between the long periods of earth-controlled and sun-controlled surface temperature. His conclusions were:

1. At the dawn of geologic time two sources of heat were active agents in the control and conservation of temperatures, (*a*) earth heat,

(*b*) solar energy converted in the upper atmosphere into heat.

2. The functions of these two sources were separate. Earth-heat controlled the surface temperatures during its prevalence, and by the laws of cooling solids was uniformly distributed at sea level; it was held near the planetary surface by the enshrouding media, by which it was trapped and through which it escaped slowly, not by direct radiation, but by the performance of work, namely the evaporation of water, and by convection currents which carried warm air to the upper regions of the atmosphere, from which regions only could free radiation of heat into space take place. Solar energy did not directly affect surface temperatures, during the existence of earth heat as a sensible factor, by reason of the intervention of a dense cloud-sphere incident to the universally warm oceans whose temperature is attested by early fossil life; but during this period solar energy acted as a conservator of planetary heat by warming the upper regions of the atmosphere and clouds.

3. Under these conditions, lower and lower temperatures supervened and were recorded by fossil life and ice action distinctly nonzonal in distribution, but varying locally through wide ranges by reason of differences in elevation.

4. Land areas reached glacial temperatures whenever and wherever they were thrust up above a snow-line controlled by earth heat, and such snow-line was in the main continuously lowered but may have fluctuated and was independent of latitude until the culmination of the ice-age; land areas reached glacial temperatures earlier than ocean areas, by reason of the low specific heat of earth and rocks, their more intense rate of radiation, through the cooling action of rain and snow and by reason of greater elevation, and they were subjected to maximum glaciation along lines of maximum precipitation, and may have escaped all but light local glaciation in regions of minimum cloud formation and precipitation.

5. Upon the cooling of the oceans, the effective remnant of earth-heat was exhausted and cloud formation reached a minimum, permitting solar energy to reach the surface and to assume domination and control of its temperatures; the climates of such control became zonal by reason of direct exposure to a zonally distributed source, and these climates gradually rose in temperature by reason of the trapping of heat rays emitted by the warming planetary surface; such rise is yet in progress as recorded by retreating glaciers and advancing plant and animal life.

6. These progressive changes of climate have been in harmony with the principles of climatic evolution herein set forth; and the principles are substantiated by the facts of geology and by the phenomena now taking place.

Mr. C. G. Abbot, of the Astrophysical Observatory, under the title 'Radiation and Terrestrial Temperature,' discussed the substantial equilibrium of temperature of the earth, and consequent equality of solar radiation absorbed in and about the earth to that emitted from and about the earth to space. After speaking of the great complexity of the earth and atmosphere as an absorber and radiator, certain maximum and minimum values of the solar constant and of the possible terrestrial temperature were obtained by considering the substitution of a black body or perfect radiator for the earth. In this way it was shown that the solar constant can not exceed 3.88 calories, and may be indefinitely below this according as the earth reflects less than 44 per cent. of solar radiation, or radiates to space less perfectly than a black body. Taking 1.9 calories as the minimum allowable assumption of the solar constant, it was shown that the mean temperature of the earth would remain above —33° C. if the earth were a perfect radiator and the reflection of solar rays did not exceed 44 per cent. Accordingly we owe not exceeding 58° rise of temperature to the imperfect radiation of the earth. But in the absence of clouds the mean earth temperature would certainly exceed 0° C. It appeared that if the temperature of the oceans could be raised 25° C. the cloudiness would so far increase as to make glaciation of the land a probable consequence. Professor Arrhenius's carbonic acid theory of glacia-

tion was discussed, and it was shown that the author of it had assumed the applicability of Stefan's law to the temperature of the gases of the air, and had neglected the dependence of connection between earth and air on the relative temperatures of the two in framing the theory. Inasmuch, therefore, as not only his fundamental equation and much of the numerical data which had been used in the computations were open to serious question, grave doubt was expressed as to the validity of Professor Arrhenius's computation of the effect of variation of the carbonic-acid contents of the air, and also of the glacial theory which Arrhenius and Chamberlin had founded upon it.

In the long discussion that followed several geologists dissented from Mr. Manson's views, holding that various important facts found no place in it. CHARLES K. WEAD, *Secretary.*

AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION.

THE first regular meeting of the season was held at the Chemists' Club, Friday evening, October 7. A number of the visiting English members of the Society of Chemical Industry were present as guests of the section. Mr. Chas. C. Cresswell, of London, the general secretary of the Society of Chemical Industry, gave a brief address, expressing appreciation of the many courtesies shown to the English members of the Society of Chemical Industry during their tour of the United States.

Doctor J. Lewkowitsch, of London, the well-known authority on oils, fats and waxes, then gave a discourse on the most recent advances in the chemistry of oils and fats. He mentioned, in the first instance, the theory of saponification of glycerides. Up till a few years ago the equation representing the hydrolysis of oils and fats, *i. e.*,

$$C_3H_5\left\langle\begin{matrix}OR\\OR\\OR\end{matrix}\right. + 3HOH = C_3H_5\left\langle\begin{matrix}OH\\OH\\OH\end{matrix}\right. + 3R\cdot OH,$$

was taken as the true expression of the reaction in the sense that one molecule of triglyceride was converted into one molecule of glycerol and three molecules of fatty acids. Geitel, however, first showed by physico-chemical experiments that the reaction underlying the hydrolysis is a bi-molecular one; according to this view the saponification of glycerides takes place in stages, the triglyceride passing through the diglyceride into monoglyceride, to be finally resolved into glycerol and free fatty acid. As the correctness of this view was doubted, he (Dr. Lewkowitsch) had tried to prove its correctness by purely chemical methods. If diglycerides and monoglycerides did appear as intermediate products in a partially saponified fat, then it should be possible to prove the presence of lower glycerides by converting the lower glycerides into a triglyceride by boiling with acetic anhydride. The then resulting triglycerides could be readily examined for the presence of acetyl groups. On carrying out the experiments, he actually proved that acetic acid was obtained on saponifying the acetylated mass. The acetic acid could be determined quantitatively in the usual manner. Now if a natural fat was saponified slowly in a manner simulating the process carried on on a large scale, and samples from the partially saponified mass were acetylated, varying amounts of acetic acid were obtained from them. On plotting the results in a system of coordinates, zigzag-like curves were observed. This proved that lower glycerides did occur in the partially saponified mass, and hence that the *progress* of saponification takes place in accordance with the view that the triglycerides pass through the diglycerides and monoglycerides, and that all three possible reactions occur simultaneously.

Dr. Lewkowitsch explained next his views on the hydrolysis of triglycerides and showed that water alone must be considered as the hydrolizing agent. The saponification processes applied in practise must, therefore, be explained by the assumption that the bases only act as catalyzers (or accelerators) and that the fundamental equation,

$$C_3H_5\left\langle\begin{matrix}OR\\OR\\OR\end{matrix}\right. + 3H_2O = C_3H_5\left\langle\begin{matrix}OH\\OH\\OH\end{matrix}\right. + 3ROH,$$

actually represents the primary chemical change. He fully explained this in the in-

stance of the saponification process by means of lime. In order to hydrolyze (saponify) a fat by means of lime in an open vat the theoretical quantity of CaO, namely, 9.7 per cent., was not sufficient to produce complete saponification, and no less than 12 to 14 per cent. were necessary to completely convert triglyceride into glycerol and the calcium salts of fatty acids. If the view were correct that the lime acts as an accelerator, then it should be possible to completely hydrolyze a triglyceride by a smaller amount of the base than theory requires for the complete saturation of the fatty acids. The theory further predicts that if the temperature and the pressure under which the reaction is carried out are increased, the amount of base may be reduced. As this is borne out by the methods practised on a large scale in which practically complete saponification is effected with from one to three per cent. of lime under pressures of twelve to eight atmospheres, the view that the bases act as accelerators was amply confirmed.

On the strength of the foregoing views one could give a rational explanation of the phenomenon of rancidity. In the first instance the triglycerides were hydrolyzed by the minute quantities of moisture which always found access to thin fats, however carefully they might be preserved. He likened the slow hydrolysis to the slow decomposition of granite in the course of centuries. The free fatty acids thus formed were then degraded in a secondary reaction into lower volatile acids, on being oxidized by the oxygen of the air, thereby acquiring that disagreeable taste and smell which we comprise under the term 'rancidity.'

Dr. Lewkowitsch next touched upon the discovery of mixed triglycerides in natural oils and fats. He first showed that theory predicted the existence of two isomeric monoglycerides, as also of two isomeric diglycerides having the same acid radical. Triglycerides, in which all three acid radicals had the same composition, could exist in only one form.

Thus in case two different acid radicals are combined with one molecule of glycerol, then theory predicted the existence of two isomerides, and if all three fatty acid radicals in one molecule of a triglyceride were different, then three different substances could exist.

The last-named class of glycerides was appropriately termed 'mixed glycerides.' The occurrence of such glycerides has been proved hitherto in a number of cases. Thus they were shown to exist in Kokum butter, cacao butter, olive oil, tallows, lard and human fat. The enormous amount of theoretically possible triglycerides opened out a wide vista of researches that would undoubtedly lead to important results in the near future.

Discussion.—In reply to Professor Bogert, Dr. Lewkowitsch said that it was almost certain that lecithin contained several fatty acid radicals in one molecule.

In reply to Dr. Ittner, Dr. Lewkowitsch agreed that optically active glycerides should exist theoretically, and he pointed to some work he had carried out over twenty years ago, when, basing himself on such theoretical considerations, he had resolved glyceric acid into two optically active components. As regards the acidic component of the glycerides, the fatty acids themselves had hitherto, with the exception of ricinoleic acid, been found optically inactive, but a few months ago Dr. Power had discovered in Chaulmoogra oil a new class of fatty acids, which were remarkable not only for containing a closed carbon ring, but also for exhibiting the property of rotating the plane of polarized light.

In reply to the chairman, Dr. Lewkowitsch pointed out that hydrolysis in alcoholic solution need not be looked upon as differing from the hydrolysis occurring in aqueous solution, if one looked upon C_2H_5 as being a basic ion. He himself thought that under pressure the conversion of a glyceride into ethylic esters might be carried out to a large extent, although, judging from the classical researches of Berthelot and Pean St. Giles he would not expect the reaction to be a complete one. The reaction may be explained by the two groups C_2H_5 and R changing places. The correctness of this view was proved by the fact, that if a fat was saponified in alcoholic solution in an insufficient amount of KOH, ethylic esters were obtained as the immediate products of

the reaction, which in their turn were hydrolyzed in a second stage with the formation of the potassium salts of the fatty acids.

The regular program of the evening was then taken up, and the following papers were read:

Inactive Thorium. CHARLES BASKERVILLE and FRITZ ZERBAN.

The authors continued the work on the complexity of thorium, in connection with the question of its radioactivity. They did not succeed, however, in entirely removing the activity from active thorium preparations, but they found originally inactive thorium in a rock from South America. This rock is gray, similar to slate, and consists mainly of barium carbonate. It does not show any radioactivity, emitting neither + nor — radiations. It contains only a small percentage of thorium and no uranium. The thorium from this source, identified by all the characteristic reactions, did not affect the photographic plate through black paper within 290 hours, nor did it give any evidence of radioactivity with the Elster and Geitel electroscope. Further investigations will be made concerning the elementary nature of this new variety of thorium, as no determination of its atomic weight has been made.

This work has been assisted by the Carnegie Institution.

The Use of Copper Sulphate as an Algacide in the Treatment of Water Supplies. DANIEL D. JACKSON.

Since the publication of the paper by Drs. Moore and Kellerman, of the Bureau of Plant Industry, U. S. Department of Agriculture, a very wide interest has been expressed in the use of copper sulphate in the treatment of water supplies. During the past five months more than fifty different sources of supply throughout the United States have been treated by this method, and the results obtained have exceeded all expectations. Both as an algacide and as a germicide the chemical is remarkable. Those micro-organisms which cause the greatest amount of trouble from tastes and odors in water supplies are the ones which are most effectively acted upon by this treatment. Also, the bacteria which are of intestinal origin are killed off with much higher dilutions than the ordinary germs.

The use of covered reservoirs to prevent microscopic growths will no longer be a necessity, as the copper sulphate treatment may be applied once or twice a year and prevent all trouble from those sources. The dilution required for such treatment is usually about one part of copper sulphate to eight million parts of water. This dilution is so great that in order to obtain a medicinal dose of the chemical it would be necessary for an individual to drink about forty gallons of water each day. After two or three days the copper is absolutely removed by precipitation.

Landscape architecture is also greatly benefited by this discovery, as unsightly and foul-smelling green growths in ponds and lakes can be easily removed and prevented. The Department of Water Supply and the Department of Parks of New York City have both had waters treated by the author during the summer with decided success, and in each case only one treatment extending for a period of one hour was necessary. The sulphate is applied by rowing a boat over the surface of the water, from the sides of which are suspended bags containing the crystals of copper sulphate. These are dissolved in the water as the boat is rowed over the surface.

The use of copper sulphate as a germicide is being very extensively experimented upon, and it is probable that typhoid fever will be entirely removed from any source of supply by a dilution of one part copper sulphate to two million parts water. In this case it is desirable to treat the contaminated stream or portions of supply at the source of the contamination, the treatment being applied at such a dilution that bacillus coli is removed.

Great credit is due Drs. Moore and Kellerman for their remarkable discovery of this treatment, which marks a decided advance in sanitary science and which undoubtedly will modify many engineering ideas regarding water supplies.

F. H. POUGH,
Secretary.

THE AMERICAN CHEMICAL SOCIETY.
NORTHEASTERN SECTION.

The fifty-fifth regular meeting of the section was held Friday evening, November 18, at the 'Tech Union,' Massachusetts Institute of Technology, with President W. H. Walker in the chair. About 150 members and guests were present. The following officers were elected for 1904–5:

President—James F. Norris.
Vice-President—Walter L. Jennings.
Secretary—Arthur M. Comey.
Treasurer—John W. Brown.
Executive Committee—Gregory P. Baxter, Edgar F. Billings, Robert S. Weston, Carl O. Weber, Lyman C. Newell.
Councillors—Charles R. Sanger, Charles L. Parsons, Albert E. Leach.

The annual reports of the secretary and treasurer were read.

President W. H. Walker opened the discussion of the subject of the 'Future Supply of Available Nitrogen,' by giving a résumé of the methods, by which the nitrogen, taken up by plants, is replaced by a fresh supply obtained from the air by chemical and electrical processes, describing the formation of calcium and barium carbamides, and the process and apparatus used for the direct conversion of nitrogen into nitric acid by using an electric arc.

Dr. George T. Moore, Director of the Laboratory of Plant Physiology of the U. S. Department of Agriculture, described the recent work carried on under his direction on the fixation of atmospheric nitrogen by means of bacteria, in which he described how the nodules formed on the roots of leguminous plants have been found to contain bacteria, which are able to fix the nitrogen of the air, and make it available for the use of the plants. These nodules are not formed naturally in all soils, owing to the lack of the presence of bacteria, and in order to make up for this deficiency, pure cultures of the bacteria in the nodules have been made and added to the soil. It was found if this was done, using the ordinary media containing nitrogen, that the bacteria were weakened, and no longer possessed the power of forming nodules, but if no nitrogen was present in the media used, the new organisms possessed the power of forming nodules to a high degree, when the seed or ground in which it was sown was inoculated with the cultures. These cultures have been dried on cotton, and distributed with the necessary food and directions for their development to about 10,000 farmers during the past year. The results so far sent in have been very satisfactory, the crop being increased in almost all instances by the use of the cultures, in some cases as high as 1,000 per cent. The lecture was fully illustrated with lantern slides, showing sections of the nodules, specimen plants, etc.

Arthur M. Comey,
Secretary.

THE ELISHA MITCHELL SCIENTIFIC SOCIETY.

The 156th meeting of the Elisha Mitchell Scientific Society of the University of North Carolina was held in the chemical lecture room, Tuesday, November 8, 7:30 p. m., the following program being rendered:

Professor J. E. Mills: 'Molecular Attraction.'
Professor H. V. Wilson: 'Experiments on the Development of the Skeleton in Sponge Larvæ.'
Professor A. S. Wheeler: 'The Theories of Dyeing with Special Reference to the Constitution of Cellulose.'

Alvin S. Wheeler,
Recording Secretary.

DISCUSSION AND CORRESPONDENCE.

STYLE IN SCIENTIFIC COMPOSITION.

The employment of a direct and perspicuous style is of immense advantage in scientific writing, perhaps more so than in other forms of literature. In scientific composition, as elsewhere, the art of writing well depends primarily upon *right thinking*, this being, as was said by Horace centuries ago, 'the beginning and fount of excellence'; and in scarcely inferior degree it depends upon *correct expression*. Concede with Pope that 'expression is the dress of thought,' and it follows that careless or faulty expression detracts as much from our appreciation of an author as slovenliness of apparel.

Superelegance of style is neither necessary nor desirable in every-day science, any more

than is fastidiousness as to the cut of working-clothes; but we have at least a right to expect accuracy of expression on the part of an author, and conformity to good literary usage, which means simply the 'consent of the learned.' Faults of beginners can be overlooked; but practised writers who plead haste as an excuse for uncomeliness are a reproach to the patricians of science, one of whom formulated the famous aphorism, '*Le style, c'est l'homme.*'

Any one can cultivate a fair style by taking sufficient pains, despising not the counsel of good masters, and, above all, by intimate association with the best authors. The classics are within reach of us all, in translated form at least, and one amongst them has never been surpassed for brevity, vividness and virility. We refer to Tacitus, of whom Senator Hoar ventures the opinion in his 'Autobiography,' that he is 'the best gymnastic training of the intellect, both in vigor and style, which the resources of all literature can supply.' His portrayal of Tigellinus, Nero's favorite, has been called 'the most damning epitaph ever penned by the hand of man'; its cold scorn is unsurpassed even by Byron's anathemas, such as make the 'Curse of Minerva' one of the most annihilating of poems.

Old Ben Jonson defines 'a strict and succinct' style as one 'where you can take away nothing without loss, and that loss to be manifest.' We learn from Ben how the best authors progressed in their beginnings: "They imposed upon themselves care and industry; they did nothing rashly; they obtained first to write well, and then custom made it easy and a habit. By little and little their matter showed itself to them more plentifully; their words answered, their composition followed; and all, as in a well-ordered family, presented itself in the place. So that the sum of all is, ready writing makes not good writing, but good writing brings on ready writing."

In geology, one of the most graceful of 'ready writers' was Hugh Miller, at whose style Agassiz marveled. Humboldt, Lyell, Darwin, all excelled in composition, as did our own Newberry; and the writings of many well-known contemporaries are models of neatness and precision. In paleontology, Cope's fecundity was at the expense of good form, his most brilliant contributions being marred, as Osborn has pointed out, by confusion of terms. This serious fault is of frequent occurrence elsewhere, and we may be permitted in what follows to devote some attention to it.

Inaccuracy in the use of terms, employment of inappropriate methods of illustration, anything in short which tends to impair the vocabulary of science and render it less efficient, are grave errors; for it is an axiom that the advancement of science depends as much upon expression as upon investigation. Yet there is one class of writers who apparently hold the matter of terminology in light esteem, the whole 'business' of technical expressions and methods of illustration being conducted contrary to the principles of good usage. Physiographers are the class of writers referred to, and we hope to accomplish some good by remonstrating mildly against certain of their improprieties.

First, as to descriptive terms. It is evident that the physiographic articles of faith include a firm belief in the penury of the English language, and unsuitability of Saxon epithets; otherwise it is impossible to explain their construction of a technical vocabulary out of a rabble of words recruited from the uttermost regions—part alien, part hybrid, part argot,—

> 'Scotch, English and slang, in promiscuous
> alliance,
> Something bad they must mean, though we
> can't make it out;—
> That they're all *anti*-English no Christian
> can doubt.'

When a foreign author can be found like Reclus, who gives in one of his works no fewer than seventy-five dialect designations for mountains, or a compatriot like Mr. Hill, who has imported whole cargos of choice Castillian (not all of the saponaceous variety)—the entire assortment is appropriated with glee. Other departments of science that can boast of but few barbarisms, such as *Gegenschein* for counterglow in astronomy, *Dreikanter* for

faceted pebbles in geology, together with *Reibungsbreccia*, *roches moutonnées*, and the like, are put to shame for their poverty. Time was when none but the untutored savage called hills 'ispatinows' and 'pahas,' and the Brahmin was left in undisputed possession of 'doabs.' In those artless days we were wont to say 'hillside' and 'pot-hole' when needful, without blushing for the uncouthness of our mother-tongue, and in blissful ignorance of such refinements as 'cuesta' and 'remolino.' But now that physiographers, like Sganarelle, 'have changed all that,' we are overcome at the thought of our unletteredness.

Sometimes, however, even Chinook and Hottentot fail, and in such plight it becomes necessary to improvise new terms on the spot. Recent coinage from a strenuous mint has enriched us with 'can't bes' and 'not yets'; may we not hope for 'has beens,' 'izzers' and 'come ons' (out west they have them already) to complete the sequence? Another bright medallion is 'inglenook,' though had the choice been left to us we would have preferred 'tiddledywink'; and there are similar jeweled expressions too numerous to mention.

Whether this sort of a polyglot-colloquial nomenclature accords well with the dignity of science we leave others to judge. But before physiographers can claim the authority of good usage they must get the majority of the people to agree with them. Ere that time comes, however, we may be permitted to set some store upon Spencer's 'Philosophy of Style,' upon Pope's great neo-classic essay, and upon the raillery of that master of satire, who says:

> 'Nor slight applause will candid pens afford
> To him who furnishes a wanting word.
> Then fear not, if 'tis needful, to produce
> Some term unknown, or obsolete in use,
> * * * * * * * * * *
> New words find credit in these latter days,
> If neatly grafted on a Gallic phrase.'

In the second place, as to methods of illustration. Readers of SCIENCE, and of John Burroughs's articles in the *Century* and *Atlantic Monthly*, are familiar with the attitude displayed by our best critics toward the popular fetich of humanizing the animal kingdom. The physiographic cult, *mirabile dictu*, aims at nothing short of humanizing the universe. The earth, to them, is a sentient being; land and sea are considered as alive; moral character an attribute of the elemental forces of nature.

Modern physiographic literature appears to be chiefly concerned with the working out of these and similar ideas *in extenso*. Everywhere is the aim to impart instruction by means of false analogy. The method of indirectness is exalted, the method of allegory, of wrong metaphor; the method that seeks to flatter minds that are quick to act on suggestion, that appeals chiefly to childish and savage intelligence. For the enlightened understanding resents having truth served up to it in spurious form. We tire of having things depicted not as they are, but grossly caricatured. In the end we lose all patience, as did old Ben when he complained that 'barbarous phrases often made him out of love with good sense, and wrong sense wracked him beyond endurance.' Then it is we wish writers would take Burroughs's advice to heart, who bids us to 'humanize facts to the extent of making them interesting, if you have the art to do it, but leave a dog a dog, and a straddlebug a straddlebug.'

Lest any suppose these strictures to be unduly severe, a few random illustrations may be offered in proof of the contrary. Certainly refined usage does not warrant the likening of topographic features to human nurslings, nor even to precocious infants,* as they have been likened. To refer a land surface to the 'puppy stage' would be ridiculous; but not one particle less so is the current series of technical terms taken from human infancy, adolescence, maturity and senility. The analogy is false, and therefore improper. Not less extravagant are the analogies taken from birth, rejuvenation and decease, including even violent death by 'drowning' or 'behead-

* The Grand Canyon of the Colorado is solemnly cited as an example of a 'precocious infant' in one of the best of modern text-books on physiography (Dryer's, 1901). Human growth-stages are even ascribed to heavenly bodies, as witness Professor Todd's account in 'The New Astronomy.'

ing,' a fate to which land and water forms are stated to fall prey. An example of a country that has apparently gone wading 'up to its knees' has been mentioned in a previous article (No. 498).

A favorite habit of physiographers is to represent rivers as wandering around with the intent of 'discovering' something, or 'attacking' or 'defending' themselves against a foe. 'Strategy' and 'tactics' are employed by a class of desperate characters known technically as 'pirates,' which, when successful, result in the 'decapitation' or 'capture' of the object of their 'assault.' The mysteries of 'sand-tactics' and 'island-tying' are revealed only to the initiated who have fully mastered this hierarchal language. We forbear to enumerate further examples, or to offer presumptuous comments on the nomenclature; but if any shall be so bold as to contend that it is dignified, and sanctioned by tradition and good taste, we may venture to entertain some doubt. C. R. Eastman.

Harvard University.

SPECIAL ARTICLES.

'BERYLLIUM' OR 'GLUCINUM.'

Since the Council of the American Chemical Society has requested the smaller International Committee on Atomic Weights to submit the question of choice between the two names 'beryllium' and 'glucinum' to the whole or larger committee in order that uniformity of usage may be secured it is evident that a considerable difference of opinion exists among American chemists as to the advisability of adopting the latter name.

The question is one of decided importance in indexing our chemical literature, and as I have had this matter brought continually to my attention during the preparation of a bibliography of the element, now complete in card form, I should like to present the very strong reasons for the universal use of 'beryllium,' at least as they appear to me. These reasons are two and may be summarized as (1) priority and (2) usage.

Priority.—It has been generally supposed by chemists who have not carefully looked into the matter that the name 'glucinum' or at least 'glucine' originated with Vauquelin, the discoverer of the element, but this is not the case. In fact a distinction should be made between the terms 'glucinum' and 'glucine' for the former first came into use many years afterward when the metal itself was obtained and the real claim for priority must be a question between 'glucine' and 'berylerde' from which the others were derived.

Vauquelin himself uses the clause 'la terre du Béril' exclusively in his first two articles on the subject in speaking of the new oxide he had discovered (*Annales de chim.*, 26, 155, and 26, 170). The term 'glucine' was proposed by the then editors of the *Annales*, Guyton and Fourcroy, in a note at the end of Vauquelin's first article and signed simply 'Redacteur.' Vauquelin evidently presented his results for the second time to the French Society of Mines, for they again appear in the *Journal des mines*, 8, 553. Here also Vauquelin uses only the clause 'la terre du Béril' but gives support to the term 'glucine' by a note at the end of his article as follows: "La propriété la plus caracteristique de cette terre étant de former du sels d'une saveur sucrée, les C^ens. Guyton et Fourcroy m'ont conseillé de lui donne le nom de glucine de (γλυκης), doux, Cette denomination sera assez significante pour aider le mémoire; elle ne prendra pas dans son étymologie un sens trop strictement determiné, et ne presentera pas d'ideas fausement exclusive, comme celles que l'on tire du nom de la pierre qui fourni le premier échantillon de la substance nouvelle, etc."

Vauquelin's adoption of 'glucine' appears from the character of the argument he puts forth to be at least half hearted. He first actually employs 'glucine' in his third article entitled 'Analyse de l'emeraude du Péron' (*Annales de chim.*, 26, 259), prefacing its use with 'on a donné le nom de glucine.'

The clause 'la terre du Béril' was translated into German as 'Berylerde' in the reprints of Vauquelin's articles and became the name used thereafter by all of the German and Swedish chemists who did much the larger portion of the work of developing the chemistry of the element.

Usage.—If 'use is the law of language' then the supporters of 'glucinum' have little upon which to base their argument. By far the larger number of investigators of the element and its compounds have used and are using 'beryllium.' All the leading chemical journals of the world with the exception of those in the French language give preference to the latter term and in most cases use it exclusively. The German, Swedish and Dutch chemists who have the greater number of original articles to their credit use no other. Italians use 'berillio' from the same root. English journals until recently used the name prefered by the particular author but they have now almost ceased to put even the 'glucinum, see beryllium' in their indexes.

For American chemists to attempt to bring the world to the use of 'glucinum' when by far the majority of chemical journals have dropped it even as a synonym is, in my opinion, worse than useless even if there was a preponderance of argument in its favor.

Charles Lathrop Parsons.
New Hampshire College,
November 11, 1904.

CURRENT NOTES ON METEOROLOGY.

CLIMATE OF BALTIMORE.

The Maryland Weather Service has issued a valuable 'Report on the Climate and Weather of Baltimore and Vicinity,' by Dr. O. L. Fassig, section director of the U. S. Weather Bureau in Baltimore, and in charge of meteorological instruction in Johns Hopkins University. This volume ('Special Publication,' Vol. II., Part Ia., 1904) was preceded, in 1899, by Vol. I., in which a report on the physiography of Maryland was followed by papers on 'The Aims and Methods of Meteorological Work, Especially as Conducted by State Weather Services,' by Professor Cleveland Abbe; 'A Sketch of the Progress of Meteorology in Maryland and Delaware,' by Dr. O. L. Fassig, and an 'Outline of the Present Knowledge of the Meteorology and Climatology of Maryland,' by F. J. Walz. The present report is modelled on the lines of climatological discussions laid down by Hann, in his 'Handbuch der Klimatologie,' Vol. I., and is the first, in point of completeness and thoroughly scientific quality, of American publications on the climatology of special areas. There are numerous graphic illustrations of the variations of the different elements, which help greatly in an adequate understanding of the conditions discussed. Each element is considered with reference to (*a*) its diurnal period, (*b*) its annual period, and (*c*) its variability, or non-periodic aspects of long and short duration. In the present issue, Part Ia, atmospheric pressure and temperature are considered. The discussion of humidity, precipitation, cloudiness and sunshine, and winds, is reserved for a later issue, now in press. Part II. will concern 'Weather.' Dr. Fassig is to be congratulated on the successful accomplishment of what has certainly been an arduous task. He has the satisfaction of knowing that he has given American climatology an impetus which it sadly needed.

CYCLONES OF THE FAR EAST.

In 1897 there was published by the Manila Observatory a monograph entitled, 'Baguios ó Ciclónes filipinos,' in which Father José Algué, S.J., director of that institution, summarized what was then known concerning the typhoons of the Philippine Islands. A German translation, by Dr. Paul Bergholz, appeared in 1900, under the title, 'Die Orkane des fernen Ostens,' and an English translation of Dr. Bergholz's translation, by Dr. R. H. Scott, was published in 1901. In an enlarged edition, with the addition of newer material, and of a more complete discussion, we now have a volume of 283 quarto pages (Manila, 1904), issued as a 'Special Report of the Director of the Philippine Weather Bureau,' and bearing the stamp, Department of the Interior, Weather Bureau. The title has been changed to 'The Cyclones of the Far East,' because the field covered is larger than that in the case of the first edition of 'Baguios ó Ciclónes *filipinos*.' Among the more notable additions to the new edition are a fourth part, dealing with practical rules for navigating, and giving a list of ports of refuge in the far east, especially in the Philippine Archipelago; a new

classification of cyclones of the far east, and the relation between the average motion of the higher clouds in the northern hemisphere and the general cyclonic tracks.

CLIMATE OF THE PHILIPPINES.

PHILIPPINE climatology is a subject which interests a considerable number of Americans, and it is well that there should be generally available an authoritative presentation of the most important facts in this connection. In the 'Report of the Philippine Commission' for 1900, there was published an account of Philippine climates, and this, with some additions and many modifications, appears in Bulletin 2, Census of the Philippine Islands, 1903, 'The Climate of the Philippines,' by Father José Algué, S.J. (Department of Commerce and Labor, Bureau of the Census, 1904, 8vo, pp. 103). The bulletin is illustrated by means of twenty-seven plates, showing graphically the variations of the different climatic elements, the tracks of typhoons, etc., and there are also two colored maps of the islands, showing the mean annual rainfall and the mean annual temperature. On the latter, four shades of yellow are employed to indicate respectively the regions of high, intermediate and mild temperature, and also cool temperature 'because of altitude.' Of course, the word *cool* is relative, and the lower temperatures of higher altitudes in the tropics do not mean seasonal changes such as are experienced in the extra-tropics, especially in the northern hemisphere. R. DEC. WARD.

GOSSIP ABOUT LAMARCK.

A MOVEMENT is afoot in Paris, to erect, probably in the Jardin des Plantes, an imposing monument to Lamarck. This will take the form of a bronze bust, or possibly a statue, surmounting a large stone base, and on the latter, after the fashion in French design (as in the newly dedicated monument to Pasteur) figures will appear in full relief. In the present case, these will represent the naturalist, blind and infirm, seated on a bench in the Jardin, and standing by his side his devoted daughter, pronouncing her memorable prophecy. And one might add that the little model of this relief loses none of its pathos when one sees it in the historic house, in the room indeed in which Lamarck suffered and died.

The proposed monument is but one of the many signs that interest in the work of Lamarck is increasing among French zoologists. M. Landrieu, au élève of Giard, has prepared a translation of Packard's 'Life of Lamarck,' but we are told that he has added to it so much material that it will appear almost as a new work. It will shortly be published as a separate volume of the Zoological Society publications (Paris). In this regard it may be added that, thanks to the cooperation of a number of French scientists, notably Professor Hamy and Professor Joubin, M. Landrieu has been able to glean many details as to the life of the French naturalist. Some of these details, it appears, have been obtained through members of Lamarck's family, his descendants having finally been traced, and, curiously enough, one has still a personal interest in the Jardin, if for no other reason than that she has married one of the professors. It appears, furthermore, that a descendant of Lamarck is at present high in station, and is indeed, if gossip be repeated, slightly annoyed at the prominence which is being given an ancestor who was in his day obscure and who was, above all things, tainted with republicanism! An interesting item, which I learned recently from one of the authorities of the Jardin, is that there probably exist many memorabilia* of the naturalist; among them, for example, is a sketch book which was known to contain many portrait sketches of him made by one of

* In this regard *cf.* SCIENCE, 1904, vol. XIX., pp. 798–800, as to a recently discovered letter signed by Lamarck and Geoffroy (1796) dealing with evolutional matters. The writer may also mention that he has in his possession what is said to be the lid of Lamarck's *tabatière*. It is ivory-rimmed, and in the glass top appears a signature of Jean Jacques, together with a specimen of *Hypnum proliferum*. The name of the plant is in Lamarck's hand, and the specimen is possibly a souvenir of Lamarck's friendship with Rousseau and of their traditional walks on Mont Valérien.

his sons. An original portrait has also been discovered painted between 1795 and 1799. *A propos* of the family of Lamarck, I noticed that the library of the Institute has preserved one of the *annonces* sent out by Lamarck on the occasion of the wedding of his son, the engineer, Auguste de Lamarck; and that, in another file, a nephew, Auguste de Longschamps, is mentioned (1825) as having been given the privileges of the library. The archives of the Institute, however, I am sorry to say, fail to show the unpublished portion of Cuvier's éloge of Lamarck: this we hope may still be forthcoming among the extensive papers of Cuvier which the library has recently acquired. The missing portion of the éloge, it need hardly be mentioned, is of special interest, since it will probably throw light on a side of Lamarck's life and work which must to no little degree have been responsible for his neglect. For the rest I may quote an explanation of the contemporary lack of appreciation of Lamarck which was made by an eminent professor at the Jardin. "Lamarck," he said, "was found to be lamentably weak in the facts upon which he based his theories, geological, chemical and meteorological: and as an immediate result his views in these fields came in course of time to be regarded as chimerical. Was it not natural, therefore, that both his friends and foes should query whether his evolutionary doctrines were better founded? His methods were thus known to be in strong contrast to those of Cuvier, who, whatever were his limitations, had at least a thoroughly modern spirit in his laborious quest for facts with which to test the relation between cause and effect."

A final item is the installation in the Jardin des Plantes of a Lamarckian museum. This has been brought together during the past year by Professor Joubin and placed appropriately in a room adjoining the malacological collection. It aims to include all specimens which are known to have passed through the hands of Lamarck. The identification of this material, which thus far consists entirely of invertebrates, has proven by no means an easy task, for original labels have frequently been displaced or lost, or covered by later labels. It is to be hoped that the authorities may see fit to extend the scope of the museum in many directions.

B. D.

THE TOTAL ECLIPSE OF SEPTEMBER 9, 1904.

A total eclipse of the sun occurred on September 9, 1904. The shadow path crossed the central Pacific Ocean from west to east without touching known land, except that it reached the coast of northern Chile six or eight minutes before sunset. Astronomer William H. Wright, in charge of the D. O. Mills expedition from the Lick Observatory to Santiago, Chile, states that Dr. Obrecht, director of the National Observatory of Chile, established an observing station at Taltal, but that the sky was cloudy at the time of totality. At Santiago the sun set, partially eclipsed, on a fine horizon.

W. W. C.

Lick Observatory, University of California,
November 29, 1904.

THE AMERICAN SOCIETY OF VERTEBRATE PALEONTOLOGISTS.

The American Society of Vertebrate Paleontologists will hold its second annual meeting in Philadelphia on December 28 to 30. By arrangement with the American Association for the Advancement of Science the meetings of the Zoological Section of the association will be held in the mornings while those of the Paleontological Society will be held in the afternoons at the same time that the Zoological Society is in session. Thus it is understood that the meetings of both societies will not conflict with the morning sessions of the American Association section, in which the papers will be of a general character.

For the Paleontological Society papers are already promised by Messrs. Osborn, Scott, Sinclair, Matthew, Merriam, Loomis and Hay. The presidential address by Professor Osborn will be entitled 'Ten Years' Progress in Mammalian Paleontology,' including a résumé of the principal discoveries of the past ten years and their bearing upon present and future problems. There will also be a discussion on the evolution and classification of the Reptilia,

in which Messrs. Williston, McGregor, Osborn and others will participate. Titles of papers should be sent to Dr. O. P. Hay, secretary, American Museum of Natural History, New York.

THE AMERICAN SOCIETY OF NATURALISTS.

THE twenty-third annual meeting of the American Society of Naturalists will be held at Philadelphia on Tuesday, December 27, and Wednesday, December 28. The following program is announced: Tuesday, 8:00 P.M., illustrated lecture by Professor Henry F. Osborn, 'Recent Discoveries of Extinct Animals in the Rocky Mountain Region and their Bearings on Present Problems of Evolution,' at the lecture hall of the Academy of Natural Sciences, Nineteenth and Race Streets; 9:00 P.M., smoker of the Affiliated Scientific Societies, University Club, Fifteenth and Walnut Streets. On Wednesday a business meeting will be held at 2 P.M. in the Laboratory of Physiology and Pathology at the University of Pennsylvania, and in the same place at 3 P.M. the annual discussion will take place. The topic, 'The Mutation Theory of Organic Evolution,' will be discussed from the following standpoints: Plant breeding, by Dr. D. T. MacDougal, of the New York Botanical Garden; animal breeding, by Professor W. E. Castle, of Harvard University; cytology, by Professor E. G. Conklin, of the University of Pennsylvania; paleontology, by Professor W. B. Scott, of Princeton University; anatomy, by Professor Thomas Dwight, of the Harvard Medical School; taxonomy, by Professor Liberty H. Bailey, of Cornell University; and ethology, by Dr. W. M. Wheeler, of the American Museum of Natural History. Each speaker is limited to fifteen minutes. At 6:45 a business meeting for the election of officers will be held at the Hotel Walton, while at 7:00 P. M. the dinner of the Naturalists, in which members of the affiliated societies may participate, will be held. At the dinner the president of the society, Professor E. L. Mark, of Harvard University, will give his address. Hotel headquarters of the society are to be at the Colonnade Hotel, Fifteenth and Chestnut Streets.

SCIENTIFIC NOTES AND NEWS.

THE trustees of the Carnegie Institution will meet at Washington on December 13, when it is expected that a president will be elected to fill the vacancy caused by the resignation of Dr. D. C. Gilman.

THE former students of Professor Charles E. Bessey who are connected with the Office of Vegetable Pathological and Physiological Investigations, Department of Agriculture, have had an enlarged copy of his photograph framed and presented to the office. The portrait, which had been covered with an American flag, was unveiled by Professor Bessey's son, Dr. E. A. Bessey. The picture was hung at a gathering of the office force on November 28. Miss Carrie Harrison presented the picture, and appropriate remarks were made by Mr. A. F. Woods, chief pathologist and physiologist, who spoke especially of Professor Bessey's work in promoting the establishment of the pathological and physiological work of the department and of his constant interest in its progress and welfare. Dr. H. J. Webber, physiologist in charge of plant breeding, spoke of the important part that Professor Bessey had taken in introducing laboratory methods of teaching botany in this country and of his great success as a teacher. Mr. C. L. Shear, pathologist, spoke briefly of his students, referring especially to those who are now holding important positions as professors of botany in various universities and colleges. All testified to the intimate and friendly relation which existed between Professor Bessey and his students and to their great admiration and affection for him.

The seventieth birthday of Dr. George H. Howison, Mills professor of philosophy in the University of California, was celebrated on November 29. A *Festschrift* has been issued by the university press containing contributions by his former pupils.

AT the last meeting of the Rumford Committee of the American Academy of Arts and Sciences the following grants for research were made: To Professor R. W. Wood, of Johns Hopkins University, $350, in aid of a research on the optical and physical properties of so-

dium vapor. To Professor N. A. Kent, of Wabash College, $100, additional, in aid of a research on the circuit conditions influencing electric spark lines. To Professor A. L. Clark, of Bates College, $150, additional, in aid of a research in the molecular properties of vapors in the neighborhood of the critical point.

Dr. J. Steindachner, director of the Natural History Museum at Vienna, celebrated on November 11 his seventieth birthday.

A chapter of the Scientific Society of the Sigma Xi has recently been organized at the University of Indiana with Dr. W. L. Bryan as president.

Mr. David Hale Newland has recently been appointed assistant state geologist of New York, as the result of civil service examinations. Mr. Newland, who will have special charge of inorganic and economic geology, graduated from Hamilton College about ten years ago, subsequently studied two years in Germany, was fellow in geology at Columbia University, assistant on the New York State Geological Survey, and for several years past has been on the editorial staff of the *Engineering and Mining Journal*.

Dr. Henry Montgomery, for ten years past professor of geology and biology and curator of the museum at Trinity University, has been appointed curator of the museum at the University of Toronto.

After the conclusion of his course of lectures at the Lowell Institute, Boston, on 'Selected Chapters in Physiography,' Professor Albrecht Penck gave three lectures before the Harvard Geological Conference on the 'Alps in the Great Ice Age,' the first considering Climatic Variations of the Ice Age (November 28); the second, Glacial Sculpture of the Alps (November 29), and the third, Man and the Ice Age (November 30). These lectures presented the chief results contained in the monograph 'Die Alpen im Eiszeitalter,' the joint work of Professors Penck and Brückner, now nearing completion. Professor Penck also gave an illustrated lecture before the Section of Geology and Mineralogy of the New York Academy of Sciences, on December 2, on 'The Surface Features of the Alps.'

Dr. Franz Boas, of Columbia University and the American Museum of Natural History, lectured at Harvard University on December 2, under the auspices of the Anthropological Society on 'Characteristics of Primitive Culture.'

At University College, London, Professor W. F. R. Weldon is giving a course of eight lectures on 'Current Theories of the Hereditary Process' and Mr. G. U. Yule is giving a course of six Newmarch lectures on 'The Vital Statistics of England.'

On November 13 the monument erected to the memory of Professor Ollier, the distinguished French surgeon, by international subscription, was unveiled at Lyons in the presence of M. Chaumié, minister of public instruction.

The Danish Parliament has voted a pension of about $1,000 a year to the widow of the late Professor Finsen.

The portraits of Professor Osborne Reynolds and Professor A. S. Wilkins, by the Hon. John Collier, were formally presented to the Victoria University of Manchester on November 18.

The death is announced of Dr. W. L. Coleman, of Houston, Texas, known for his work on yellow fever.

The cornerstone of the central building of the Rockefeller Institute for Medical Research was laid on December 3 by Dr. Simon Flexner, director of the institute. The building, which is at Sixty-fifth Street and Avenue A, will have a frontage of one hundred feet, will be five stories in height and will cost $345,000.

At the monthly meeting of the Zoological Society of London it was announced that the total number of visitors to the society's gardens during the months of August, September and October had been 256,630, showing an increase of 7,236 as compared with the corresponding period in 1903.

The New York *Evening Post* states that preparations are being advanced for the expeditions to be sent by the University of California, through the aid of Mr. William H. Crocker, to observe the next total eclipse of

the sun. Parties will go from the Lick Observatory to Spain, Labrador and Egypt. Professor Svante Arrhenius, of Stockholm, a member of the faculty for the last summer session, and Professor Wilhelm Ostwald, of Leipsic, will join the expedition which will go to Spain under the personal charge of Director W. W. Campbell.

PROFESSOR CHITTENDEN, director of the Sheffield Scientific School, has announced a gift from George J. Brush, late director of the institution and emeritus professor of mineralogy, of his valuable collection of minerals and of his scientific library, chiefly of mineralogical books and journals. In addition is given a fund of ten thousand dollars, the income of which is to be used for the increase and care of the collection and library. The value of the entire donation is estimated at about forty thousand dollars. The Brush collection of minerals, which has been recently placed in Kirtland Hall, is the result of over fifty years of judicious and painstaking selection of choice specimens from nearly all parts of the world. Much of it was collected by the donor himself. It has been formed especially to illustrate the scientific aspects of the subject of mineralogy and for study and investigation. It is particularly rich in the original type specimens of new minerals and in other material which has been investigated and published upon. For this reason it is known to scientists and collectors everywhere as having an especial value, and it is a matter of congratulation among friends of the institution that, through the kindness of the donor, this famous and valuable collection has been secured for the perpetual use of the school. It will be henceforth under the charge of Professor S. L. Penfield, who will act as curator.

THE department of geology of Bryn Mawr College has recently been presented, by Mrs. Charles Stillwell Eldredge, daughter of the late Theodore D. Rand, of Radnor, Pa., with her father's private rock and mineral collections. Mr. Rand, for thirty-one years treasurer of the American Institute of Mining Engineers, was an enthusiastic and discriminating collector of minerals and had secured, at the time of his death, some twenty to thirty thousand specimens. Among them are many rare minerals seldom found in private collections and many valuable and interesting crystals and mineral pseudomorphs. The rock collection is limited in geographic distribution to the United States and is chiefly illustrative of the crystalline rocks of eastern Pennsylvania. It includes a fine series of polished serpentines and rock-type, to which Mr. Rand had devoted considerable study. Mr. Rand's monograph on 'The Geology of Eastern Pennsylvania' is published in the *Reports of the Second Geological Survey* (Annual Report, 1886, Part IV.) and in the *Proceedings* of the Philadelphia Academy of Sciences.

THE New York State Museum received the following awards at the St. Louis Exposition: Four grand prizes for general exhibit in the education department, paleontology, salt products, gypsum. Six gold medals for general scientific publications, minerals and building stones, cement, salt, iron ore separator, electrical insulators. Eleven silver medals as follows: Four for salt exhibits; two for sandstone exhibit; one for each of the following collective exhibits: Geological maps, granite, iron ore, clay products. Three bronze medals for exhibits of marble, iron ore and plaster model of iron mine.

St. Louis University, the oldest university in the Louisiana Purchase territory, has been awarded three grand prizes at the St. Louis World's Fair, as follows: (1) for the general exhibit; (2) for original drawings in embryology by Professor A. C. Eycleshymer; (3) for twenty-five charts of topographical anatomy by Professor Peter Potter. Several gold and silver medals were also awarded.

As already announced, the fourth meeting of the American Philosophical Association will be held in Philadelphia, December 28–30, in affiliation with the American Psychological Association, the American Society of Naturalists and other societies convening with the American Association for the Advancement of Science. In accordance with a vote at the last meeting, arrangements have been made, in commemoration of the centenary of

the death of Kant, for a series of papers on 'Kant's present significance.' There will also be a paper in recognition of the bicentenary of the death of Locke. It is hoped that one session may be held conjointly with the Psychological Association. In addition to these special features, a full and varied program is assured by the large number of papers already offered.

THE San Francisco section of the American Mathematical Society holds its meetings alternately at the University of California and Stanford University, in September and February, respectively. The next meeting will be held at Stanford University on February 25. Professor M. V. Haskell is president and Professor G. A. Miller, secretary of the section.

AT the recent Princeton meeting of the Association of Teachers of Mathematics of the Middle States and Maryland, officers were elected as follows: *President,* Professor Thomas Scott Fiske, Columbia University; *vice-president,* Dean H. B. Fine, Princeton University; *secretary,* Arthur Schultze, High School of Commerce, New York City; *members of the council,* Professor Edwin S. Crawley, University of Pennsylvania, and Miss Jenny Van Vleck, Girls' High School, Brooklyn.

THE American Society of Mechanical Engineers is meeting in New York this week. Mr. Ambrose Swasey, the retiring president, announced as the subject of his address 'The Achievements of the Engineer with respect to exact Measurements.' Mr. John R. Freeman, of Providence, R. I., has been nominated for president next year.

THE opening meeting of the eighty-sixth session of the British Institution of Civil Engineers took place on November 1. Sir William White, the retiring president, took the chair at the opening of the meeting, and he was supported by Sir Guilford Molesworth, the incoming president, Sir Benjamin Baker, Sir William Preece, Professor Unwin, Colonel Crompton, Sir J. Wolfe Barry, Mr. Alexander Siemens, Dr. Kennedy, Sir Alexander Binnie, Mr. C. Hawksley, Mr. J. C. Hawkshaw, Dr. Elgar, Mr. Yarrow, Sir John Thornycroft, Sir George Bruce, Mr. R. A. Hadfield and Dr. Tudsbery (secretary).

THE Ben Nevis Meteorological Observatories were closed on October 1, after having been in operation for nearly twenty-one years. The cost of the observatories and improvements was about $35,000 provided by subscription, and the annual cost of maintenance had been about $5,000, most of which was privately subscribed.

UNIVERSITY AND EDUCATIONAL NEWS.

THE New York *Evening Post* states that Sir William Macdonald is perfecting his plans for the new agricultural college and experimental farm which, under the supervision of Professor Robertson, late of the Government Experimental Farm at Ottawa, he is to establish at St. Anne de Bellevue, near Montreal. About seven hundred acres of land have been secured by the founder, who declares that the college is a personal affair of his own, and that he does not intend to be bound down to any definite sum of money in the completion of his work. The erection of the buildings will be commenced next spring, and the whole cost is expected to be between one and two million dollars. The institution will be patterned after the agricultural college at Guelph, and the experimental farm at Ottawa.

THE Oxford Congregation by a vote of 200 to 164 has rejected the proposal that in the entrance examinations candidates who are seeking honors in mathematics or natural science may be allowed to substitute French or German for Greek.

AT a dinner of the Oxford Colonial Club, held on November 14, Lord Roseberry announced that the Rhodes Trustees, hearing that the university was in some danger, from want of the necessary means, of losing their present teacher of pathology, were prepared to contribute £200 a year for five years.

DR. LEO LOEB has been appointed assistant professor of pathology at University of Pennsylvania.

MR. R. G. D. RICHARDSON has been appointed instructor in mathematics at Yale University.